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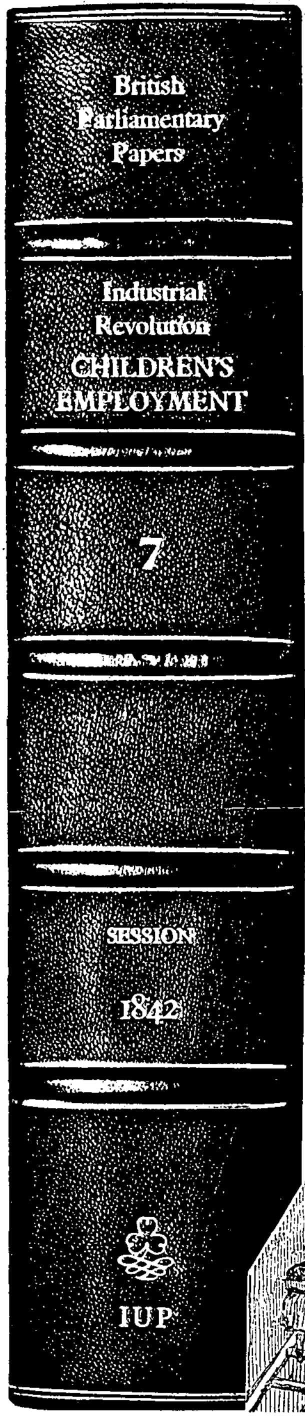

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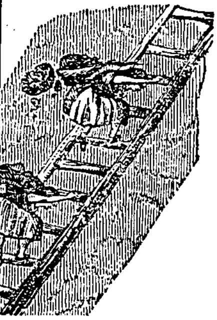

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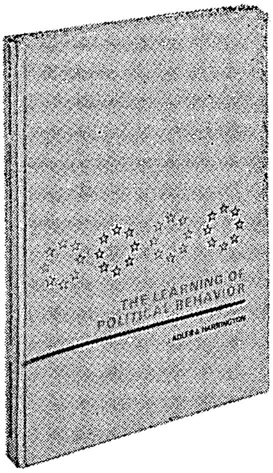

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The American Political Science Review

VOL. LXIV MARCH, 1970 NO. 1

# POLITICAL AMBITIONS, VOLUNTEERISM, AND ELECTORAL ACCOUNTABILITY*

KENNETH PREWITT
*University of Chicago*

A generally accepted interpretation of American politics today is associated with the "theory of electoral accountability." The salient features of this theory are well known. The thesis was initially shaped in Schumpeter's classic work on democracy, and since has been elaborated by a generation of scholars.[1] The elaboration, especially where grounded in empirical studies, has established (1) that the public, being largely apathetic about political matters and in any case ill-informed regarding public issues, cannot provide the necessary and sufficient conditions for the maintenance of democratic procedures; (2) that a liberal political and social elite are committed to the preservation of democratic forms, at least more committed than the average citizen; therefore, (3) what maintains the democratic tradition is not extensive public participation in political policy-making, but, instead, competition among elites whose behavior is regulated by periodic review procedures. Competition among elites and review by citizens of political leaders are provided by elections. Thus elections hold political leaders accountable to nonleaders.[2]

Writers associated with this general position have recently come under scholarly attack. The critique, directed at the first two assertions, can be reviewed briefly: although true that the public is not well-informed politically and is not actively engaged in political life, this is not to be attributed to the inherent traits of citizens so much as to the structure of political opportunities in the United States.[3] Moreover, although true that research has detected among political leaders a greater commitment to democratic procedures than is the case for the ordinary citizen, this commitment is to procedures in which only the leaders participate. It is a commitment neither to substantive justice nor to widespread political participation.[4] The critics propose that the facts of American politics have been misinterpreted in such a manner as to disguise the extent to which democracy falls short of its promise. Further, in misinterpreting the facts, contemporary political science actually impedes the realization of a more democratic politics.[5]

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* The larger project of which this analysis is a part, the City Council Research Project, is sponsored by the Institute of Political Studies, Stanford University, and is supported by the National Science Foundation under grants GS 496 and GS 1898. I am indebted to several colleagues who read and sharply criticized an earlier version of this paper. I leave them unnamed for it is very possible they would prefer not to be associated with even this version.

[1] Joseph Schumperter, *Capitalism, Socialism and Democracy* (New York: Harper & Row, 1947).

[2] Relevant studies are Bernard Berelson, *et al., Voting* (Chicago: University Press, 1954), especially chapter 14; Robert A. Dahl, *Preface to Democratic Theory* (Chicago: University of Chicago Press, 1956) and Dahl, *Who Governs?* (New Haven: Yale University Press, 1961); V. O. Key, *Public Opinion and American Democracy* (New York: Knopf, 1961), especially chapter 21; S. M. Lipset, *Political Man* (New York: Doubleday & Co., Inc., 1960, Anchor Books edition, 1963); David Truman, "The American System in Crisis," *Political Science Quarterly,* 73 (December, 1959, pp. 481–97).

[3] Important statements are to be found in Peter Bachrach, *The Theory of Democratic Elitism* (Boston: Little, Brown and Co., 1967); and Jack L. Walker, "A Critique of the Elitist Theory of Democracy," this REVIEW, Vol. LX (June, 1966), 285–95. A data article consistent with the thesis is Walter Dean Burnham's "The Changing Shape of the American Political Universe," this REVIEW, 59 (March, 1965), 7–28. A book which anticipates much of this critical literature is T. B. Bottomore's *Elites and Society* (London: Penguin Books, 1964), especially ch. VI.

The critical exchange about the propositions and findings associated with the theory of accountability has given little attention to the third assertion, that elections hold the few who govern accountable to the many who are governed.[6] This is surprising, for with respect to democratic theory a proposition linking elections and accountability is necessarily a central one. Ours is an inquiry into the conditions under which elections do insure accountability.

## I. ELECTIONS, AMBITIONS, ACCOUNTABILITY

A very few leaders govern the very many citizens. Although the many are often politically ignorant and apathetic, the few who govern are nevertheless responsive to the preferences of the many *because,* as elected officials, the few can be and are held accountable for their actions. Accountability is assured because men want to gain and to continue in office and because these men recognize that the voting public determines who will hold office. Therefore, since the electorate through periodic elections grants or withholds the privilege of governing, men who wish to govern select policies they believe to be in accord with voter preferences.

This thesis has an honorable tradition. Schumpeter's famous definition of democracy is a useful beginning point: "The democratic method is that institutional arrangement for arriving at political decisions in which individuals acquire the power to decide by means of a competitive struggle for the people's vote."[7] In his most frequently cited essay on democracy Lipset writes of a "social mechanism which permits the largest possible part of the population to influence major decisions by choosing among contenders for political office."[8] Or, as more succinctly stated by Downs, contestants for office "formulate policies in order to win elections, rather than win elections in order to formulate policies."[9] Dahl has been the most influential writer in the Schumpeter tradition. In a theoretical statement about democracy he notes that the effective political elites operate within limits "set by their expectations as to the reactions of the group of politically active citizens who go to the polls."[10] This hypothesis is generally confirmed in his empirical study of New Haven; "elected leaders keep the real or imagined preferences of constituents constantly in mind in deciding what policies to adopt or reject."[11]

The works of Schumpeter, Lipset, Downs, and Dahl, have been very influential among scholars theorizing about democracy. It has been the accomplishment of these scholars to examine a political process which provides a measure of citizen control over the few who are selected as political leaders. Thus the somewhat banal observation that fewer men issue commands than must obey them is converted into the more powerful theory that elections hold those few accountable for the commands they issue.

The theory of electoral accountability can be reviewed from another, complementary perspective to the one already suggested. Carl Freidrich's "rule of anticipated reactions"[12] provides the hint for this other perspective and Schlesinger's "ambition theory of politics"[13] makes it explicit. Men in office anticipate the likely response of voters because the incumbents want either to retain that office or to move to a more elevated one. This perspective on accountability assumes that there is a supply of men intent on gaining and then holding political office. This assumption links directly to thinking about accountability. For if men are uninterested in gaining and holding office, why should they guide their actions in a manner sensitive to voter preferences?

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[4] It has been the substantial accomplishment of Geraint Parry to explicate this point. He writes that although elites may be disagreed on policy, "they may share similar views as to the appropriate decision-making process, namely negotiation between elites." *Political Elites* (London: George Allen and Unwin Ltd., 1969), p. 90. The complete implications of Parry's point cannot be developed at this time. It is evident, though, that elites can simultaneously be committed to democratic norms *and* to a process of decision-making in which only they participate. It should be remembered that the data base for arguing that political leaders are committed to democracy is primarily survey items on civil liberties and not items on the wisdom of the participation of the "average" critizens in the decision processes.

[5] I do not mean to imply that the scholars listed in footnotes three and four necessarily associate themselves with this critique of the political science discipline.

[6] An exception to this is Murray Edelman who has very creatively called into question some of our assumptions about what elections do and do not do. See in particular the first chapter of *The Symbolic Uses of Politics* (Urbana: The University of Illinois Press, 1967).

[7] Schumpeter, *op. cit.,* p. 269.

[8] S. M. Lipset, *op. cit.,* p. 27.

[9] Anthony Downs, *An Economic Theory of Democracy* (New York: Harper & Row, 1957), p. 28.

[10] Dahl, *Preface, op. cit.,* p. 72.

[11] Dahl, *Who Governs?, op. cit.,* p. 164.

[12] Carl J. Freidrich, *Man and His Government* (New York: McGraw-Hill, 1963), pp. 199–215. Of course Freidrich's "rule" is much broader than what is implied by my use; he develops a theory of political influence based on the rule which refers to nearly all political interactions, not just to relationships between electorates and representatives. A study which applies the "rule of anticipated reaction" and comes to conclusions similar to those

Schlesinger has stated this point vigorously. "Ambition lies at the heart of politics," writes Schlesinger. He reasons, with particular relevance to democratic theory,

> To slight the role of ambition in politics, then, or to treat it as a human failing to be suppressed, is to miss the central function of ambition in political systems. A political system unable to kindle ambitions for office is as much in danger of breaking down as one unable to restrain ambitions. Representative government, above all, depends on a supply of men so driven; *the desire for election and, more important, for reelection becomes the electorate's restraint upon its public officials.* No more irresponsible government is imaginable than one of high-minded men unconcerned for their political futures.[14]

We can now reformulate the theory of electoral accountability as we understand it.

> Since periodic elections hold officeholders accountable, these officeholders select policies in anticipation of voter response and thus choose policies which broadly reflect the preferences of the governed.

This thesis, in turn, rests upon a prior assumption:

> Men in political office work to gain their positions and wish to retain them.

### II. SOME PUZZLING RESPONSES

In a study of the men who govern eighty-two cities of the San Francisco Bay Area, more than 400 city councilmen were interviewed with a lengthy and largely open-ended questionnaire. A wide variety of topics were covered, and councilmen were given ample opportunity to state the decision criteria they invoked as they set about the task of governing their communities. In reading these protocols it becomes evident that often the sentiments voiced by these councilmen do not match well with what the "rule of anticipated reactions" leads us to predict. Indeed, if the reader of the protocols is bearing in mind the observation that elected leaders "formulate policies in order to win elections," then the responses can only be described as puzzling. For it is very clear that councilmen infrequently refer to elections and, when they do, it often is in a manner which directly contradicts the premises of a theory of electoral accountability.

For example, councilmen were asked if they felt it were easy or difficult to go against majority preferences when choosing community policies.

> Easy, I am an independent type of individual. I don't feel the weight of voter responsibility. I am not all fired up for a political career.

> You don't always follow the majority; you shouldn't give a damn whether you get elected or not. Don't be afraid to be defeated.

> Yes, it is easy because I don't really care if I get elected or not.

> I am free to do as I feel. In general it is easy to vote against the majority because I don't have any political ambitions.

Responding to a related question, another councilman explains why he and his colleagues ignore the pressures which mount from community influentials:

> I doubt if the City Council would consider preferentially the opinion of such a person. This City Council doesn't give a damn if it is reelected.

More of the same is voiced in response to questions about the councilmen's political futures. One respondent, not sure whether he will stand for reelection, makes his position clear.

> I won't know until my time is up. I don't think a councilman can do a good job if he is concerned about counting votes. If something is best for the city, you have to go against some groups. And you don't want to have to worry that these groups may not vote for you in the next election if you vote wrong.

Another doubtful candidate,

> I promised myself I wouldn't decide ahead of time whether to run or not. I don't want to do things to collect votes.

These quotations, although clearly contradicting the theory of electoral accountability, do not constitute a data base on which to confirm or

---

advanced here is Roy Gregory, "Local Elections and the 'Rule of Anticipated Reactions,'" *Political Studies,* XVII (March, 1969), 31–47.

[13] Joseph A. Schlesinger, *Ambition and Politics* (Chicago: Rand McNally & Co. 1966).

[14] *Ibid.*, p. 2, italics added.

falsify anything. For one thing, the quotations were selected to make a point. Moreover, they are responses from one small population of elected officials—nonsalaried, nonpartisan city councilmen from one region of one state. Yet these responses should not be dismissed too readily. The manner in which councilmen reflect on their relationships with the public indicates that the rule of anticipated reactions does not always operate. It is in order to ask whether there are political conditions which, if present, would make these councillor responses less puzzling. This inquiry is best initiated by specifying more exactly a proposition in the theory of electoral accountability.

### III. THE SELECTION AND DISPLACEMENT OF POLITICAL LEADERS

A very crude classification permits categorizing theories of democracy as having one of two emphases. One emphasis stresses widespread citizen participation in the making of public policy. Of relevance to scholars in this tradition are issues such as expansion of suffrage, citizenship training, methods of citizen petitioning, rights of association and free speech, mobilization of the public, and so forth. The second emphasis stresses procedures of leadership selection and displacement. Relevant issues are extent of organized competition for office, availability of regularized review procedures, tenure limitations, access of all social groups to political recruitment channels, circulation of elites, and so forth. Though no scholar is faced with an either-or choice, individual scholars as well as schools of thought tend to concentrate on one or the other set of issues.

The theory of electoral accountability largely directs attention to analysis of leadership selection and displacement. Schumpeter takes "the view that the role of the people is to produce a government." He continues, "it should be observed that in making it the primary function of the electorate to produce a government (directly or through an intermediate body) I intended to include in this phrase also the function of evicting it. The one means simply the acceptance of a leader or a group of leaders, the other means simply the withdrawal of this acceptance. . . . electorates normally do not control their political leaders in any way except by refusing to reelect them or the parliamentary majorities that support them."[15] Lipset advances a similar argument: "Democracy, however, implies permanent insecurity for those in governing positions; and the more truly democratic the governing system, the greater the insecurity of the incumbent. Thus every incumbent of a position of high status within a truly democratic system must of necessity anticipate the loss of his position by the operation of normal political processes."[16] The processes which provide for democracy are sustained competition for political office and regularized procedures for review by electorates of leaders; parties establish the former, and elections establish the latter of these conditions.

TABLE 1. PROPORTION OF COUNCILS WITH VARIOUS APPOINTMENT RATES (N=81)

| Percentage of Councilmen Initially Appointed to Office (N=81) | Percentage of Cities With Different Appointment Rates |
|---|---|
| Fewer than ten | 19% |
| 10 to 19 | 16 |
| 20 to 29 | 23 |
| 30 to 39 | 11 |
| 40 to 49 | 10 |
| 50 to 74 | 14 |
| 75 to 100 | 7 |
| | 100% |

Since the present task is to explain some "puzzling" responses of elected officials, attention should turn to the procedures for selecting and displacing these officials. The study of the city councils reveals four relevant facts, two related to the selection of the legislators and two related to the manner in which they are displaced.

1) *The Frequency of Appointment to Elected Office.* The theory of electoral accountability makes the tacit assumption that persons holding elected office earned them by competing for voter support. This need not be the case; nearly one-fourth (24 per cent) of the city councilmen interviewed had initially reached office by being appointed to fill an unexpired term. Such frequent use of a method of leadership selection which circumvents the election process will undoubtedly affect how the leaders, appointed as well as those who do the appointing, think about the sanctioning power of elections. Table 1 shows the frequency with which the appointment strategy is used among the cities in the study. These data are taken from aggregate election statistics over a ten-year period, five elections for each city. (Data are available for

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[15] Schumpeter, *op. cit.*, p. 269, 272.

[16] S. M. Lipset, Martin Trow, and James Coleman, *Union Democracy* (Garden City: Anchor Books, Doubleday & Co., Inc., 1962), p. 241.

81 of the 82 cities in the study.) In seventeen cities half or more councilmen are initially appointed to office. More than four-fifths of the cities have at least one member appointed (councils are never fewer than five members). In cities where appointment is a regularly employed method of selecting some or most leaders, there may occur a nonchalance about elections.

2) *The Electorate as a (Small) Constituency.* It is well known that relatively few citizens express their preferences in local elections. This generalization holds true for the cities we are studying. Averaged over five elections, fewer than one in three of the adult citizens cast a ballot in the councilmanic elections. Although low voter turnout is normal, what is interesting is to consider the interaction between low turnout and the plurality method of counting ballots. The cities being studied are all nonpartisan and use some variant of at-large election procedures. Depending on the number of vacancies, the two or three candidates with the most votes win. Except in a very few cases, there are no run-off elections. Low turnout and plurality elections mean that a very small number of citizens can elect a candidate to office. Table 2 presents illustrative data showing, for five different size cities, the *minimum* number of votes it took to gain a council seat averaged for five elections. For instance, in a city of more than 13,000 residents, on the average as few as 810 voters elected a man to office. Such figures sharply question the validity of thinking that "mass electorates" hold elected officials accountable. For these councilmen, even if serving in relatively sizable cities, there is no "mass electorate"; rather there are the councilman's business associates, his friends at church, his acquaintances in the Rotary Club, and so forth which provide him the electoral support he needs to gain office.

TABLE 2. MINIMUM NUMBER OF VOTES NEEDED TO WIN OFFICE IN DIFFERENT SIZE CITIES

| City Population (1964 estimate) | Average Total Vote of Lowest Winner—Five Elections* (1955–65) |
|---|---|
| 71,000 | 5,298 |
| 28,750 | 1,931 |
| 13,450 | 810 |
| 6,675 | 518 |
| 500 | 64 |

* To compute the average total vote of the lowest winner, we simply added the votes of lowest winner for five elections and divided by five. Except in special elections, there are never fewer than two winners for any council election.

TABLE 3. PROPORTION OF CITIES WITH VARIOUS INCUMBENT WIN RATIOS (N = 81)

| Percentage of Incumbents Who Successfully Sought Reelection | Percentage of Cities With Different Incumbent Win Ratios |
|---|---|
| 90 to 100 | 30% |
| 80 to 89 | 20 |
| 70 to 79 | 19 |
| 60 to 69 | 15 |
| 50 to 59 | 7 |
| 25 to 49 | 6 |
| 0 to 24 | 3 |
| | 100% |

These two observations, one pointing out the frequency of appointments and the other suggesting that the effective electorate for any given officeholder can be very small in local, plurality elections, give pause to interpretations of politics which emphasize that selection to office is controlled by large electorates indirectly voicing policy preferences. In the present research site, selection of leaders is less controlled by a voting public than might be necessary for the conditions of electoral accountability to hold. What about the electorates' power to evict the governors?

3) *The Infrequency of Election Defeat.* Implicit in the theory of electoral accountability is the assumption that voters frequently enough remove men from office that election defeat is seen by officeholders as a threat to tenure. It is difficult to know how frequent is "frequent," but the success of incumbents at gaining reelection is commonly taken for granted by students of American politics. Over the ten-year period, four out of five incumbent councilmen who stood for reelection were successful. This figure, though high, is even somewhat lower than one reported for members of the House of Representatives. During the years 1924–1956, 90 per cent of the congressmen who sought reelection were returned by the voters.[17] Once in elected office men can feel relatively secure about staying there. Indeed, in 20 of the 82 cities studied, over a ten-year time period not one single incumbent seeking another term failed to be reelected. Table 3 presents pertinent data.

[17] David A. Leuthold, *Electioneering in a Democracy* (New York: John Wiley & Sons, 1968), p. 127.

Two speculations can be derived from this table, both of which run counter to the assumption that elections insure accountability. The frequency with which officeholders are returned to office suggests that they are unlikely to be constantly preoccupied with voter preferences. Moreover, the voting public, which already constitutes a minority of the eligible population, evidently does not see in elections the means by which to force leadership turnover.

4) *The Frequency of Voluntary Retirement from Elected Office.* In the cities we are studying, more men by far depart from elected office voluntarily than because of election defeat. Officeholders simply conclude that the obligations of office exceed the rewards. Among the nearly 500 men who govern the cities of the San Francisco Bay Area, about one-fifth will be planning to retire voluntarily at the end of the present term and another 30 to 50 per cent will be planning to retire after only one more term. It is not the electorate jealously guarding its power to sanction officeholders which establishes tenure limitations. Rather, there are widely shared norms among these councilmen which lead to self-imposed limitations. The single most important reason for voluntary departures is simply "I've been on long enough" or "I have done my duty." As one typical councillor states it:

> No, I won't run again. I think this is the end. If I had my way, I'd limit it to two terms anyway. You get a lot of new blood that way. New ideas are needed and new councilmen can provide them. Give the next guy a chance.

Thus it is that norms held by the councilmen themselves often lead to turnover among officeholders, not the behavior of an aroused or impatient electorate. Table 4 presents data.

TABLE 4. PROPORTION OF CITIES WITH VARIOUS VOLUNTARY DEPARTURE RATES (N=81)

| Percentage of Councilmen Who Left Office Voluntarily | Percentage of Cities With Different Voluntary Departure Rates |
|---|---|
| 90 to 100 | 1% |
| 80 to 89 | 5 |
| 70 to 79 | 9 |
| 60 to 69 | 28 |
| 50 to 59 | 22 |
| 40 to 49 | 11 |
| 30 to 39 | 14 |
| 20 to 29 | 7 |
| fewer than 20 | 3 |
| | 100% |

### IV. THE NORM OF VOLUNTEERISM

This brief attention to the manner in which men enter and depart elected office in 82 California cities points uniformly in one direction. The quotations earlier reported, though puzzling when read in terms of electoral accountability perspectives, are not puzzling when read in the context of the cumulative impact of (1) the frequent reliance on appointment as a route to office, (2) the smallness of the electorate which can send and retain a man in office, (3) the overwhelming electoral advantage held by the incumbent, and (4) the high rate of voluntary retirement from office. Men enter and leave office not at the whim of the electorate, but according to self-defined schedules.

To the extent this is so, it is likely that a strong norm of volunteerism is at work in these city councils. This appears to result directly from the process by which councilmen are recruited into office, the images they hold of council service, and the manner in which they vacate office. They treat council service as a "citizen duty" in much the same manner as they treat service in the Chamber of Commerce, the PTA, the Library Board, and other such community service organizations. As we pointed out elsewhere, the norm of volunteerism "can serve to undermine an already weakened election system. Although the volunteer in office, especially if relatively indifferent to staying there, may be a devoted public servant as he defines the role, he is unlikely to be constantly sensitive to voter preferences. His political thinking has been formed by a series of experiences which minimize for him the importance of mass electorates."[18]

#### *Volunteerism and the Theory of Electoral Accountability*

It is clear why electoral accountability is weakened to the extent that volunteerism dominates in a political community. Both the choosers and the chosen come to think of movement into and out of political office as being regulated by self-selection and self-elimination patterns rather than electoral challenges. We have shown elsewhere that in communities where elections seldom evict governors, the officeholders tend to act in response only to their own image of what the community needs. The council which does

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[18] Kenneth Prewitt, *The Recruitment of Political Leaders: A Study of Citizen-Politicians* (Bobbs-Merrill, in press). This book includes a more detailed analysis of the survey data.

TABLE 5. RELATIONSHIP BETWEEN EXTENT OF VOLUNTEERISM IN LEADERSHIP SELECTION AND SEVEN DEPENDENT VARIABLES RELATED TO THE THEORY OF ACCOUNTABILITY

| Attribute of Council | Extent of Volunteerism* | | |
|---|---|---|---|
| | Most Present (15) | Mixed (49) | Least Present (17) |
| a. Council votes with perceived majority public opinion. | 35% | 49% | 53% |
| b. Council senses demands from the public. | 29% | 51% | 73% |
| c. Council reflects concern with next election. | 29% | 45% | 60% |
| d. Council activates constituencies in connection with policy-making. | 35% | 43% | 73% |
| e. Council views many groups as politically influential. | 29% | 51% | 67% |
| f. Council facilitates group access. | 35% | 61% | 60% |
| g. Council performs services for constituents. | 29% | 41% | 67% |

* The councils scored as "most volunteerism" are those above the median of all councils with respect to frequency of appointment to the council and rate of voluntary retirement; those scored as "least volunteerism" are those below the median on these two measures; the "mixed" councils are high on one of the measures but low on the other.

not face the threat of election defeat is less likely than a council which does to respond either to attentive publics or to ad hoc issue groups.[19] We can now carry this analysis a step further, by classifying cities with respect to the "norm of volunteerism."[20]

For present purposes, two indicators of volunteerism are used: the frequency of appointment as a route to elective office and the frequency of voluntary departure from office. Cities were ranked high to low on each of these measures; they were then divided at the median and cities above the median on both measures are said to be the most characterized by the norm of volunteerism. Cities below the median on both measures are least characterized by this norm. Cities high on one measure but low on the other are for present purposes simply left as intermediate. Two indicators, then, are combined to construct an ordinal measure of volunteerism.

The hypothesis is very simple: councils characterized by volunteerism will exhibit traits which contradict what the theory of electoral accountability predicts about elected legislatures. Table 5 confirms this hypothesis. Where the norm of volunteerism is more prevalent, councils are (a) more likely to vote against what they see as majority opinion, (b) less likely to feel under pressure from the public, (c) less likely to consider the upcoming election when choosing among policy alternatives, (d) less likely to involve constituencies as part of the strategy of policy-making, (e) less likely to view political groups as having an influential part to play in city politics, and (f) less likely to facilitate group access to the council, and (g) less likely to perform services for constituents.[21]

Each item in Table 5 merits discussion, for each one relates in its own manner to a theory of electoral accountability. For instance, if the anticipated reactions of the voting public are thought to be relevant to policies chosen by governors, then elected councils will likely involve constituencies as the council carries out its deliberations. Yet it is clear that councils characterized by volunteerism seldom mobilize constituencies as part of the activities which surround the making of policy. Or, another example, elected councils will feel under some pressure from publics if indeed the councils are keeping the preferences of constituents in mind as they govern their cities. But we note that fewer than one-third of the more volunteeristic councils report being under such pressure. And so forth.

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[19] Kenneth Prewitt and Heinz Eulau, "Political Matrix and Political Representation," this REVIEW, LXIII (June, 1969), pp. 427–441.

[20] In this analysis the council in contrast to the individual councilman is taken as the unit of analysis. The theoretical rationale for this as well as some methodological considerations are spelled out in Prewitt and Eulau, *ibid.*

[21] In every case the dependent items in Table 5 derive from some method of aggregating the responses of individual councilmen into a group measure. Two procedures are represented. With respect to all items but 'c' and 'd' the responses of individual councilmen were summed, a mean was computed, and the distribution of means was then used to assign councils as either above or below the median of all councils. With respect to the remaining items, a procedure relying on coder judgment to assign a score to each council was used. Footnote 9 in Prewitt and Eulau, *ibid.*, describes the procedure.

It would detract from the central argument to pursue every theoretical lead suggested by Table 5. Instead, a general summary observation can be advanced. The theory of accountability is both confirmed and confounded by the evidence of Table 5. On the one hand, councils in cities where elections play a greater role in the selection and eviction of governors appear to be more sensitive to public constituencies. On the other hand, there are processes at work, summarized by the term volunteerism, which weaken the capacity of elections to insure the accountability of the elected legislature to the voters.

This summary inference from Table 5 suggests, in turn, several ways in which to speculate about volunteerism, elections, and democratic accountability. First, however, it is important to raise the nagging question: How unusual are the Bay Area councils with respect to the norm of volunteerism?

### *Scattered Evidence About the Distribution of Volunteerism in American Politics*

Although the measures used in this study forced some variation among the cities, it is plausible that if all political units in the U.S. could be arrayed along a continuum from the most to the least volunteeristic, the 82 units here studied would cluster well toward the "most volunteeristic" end. Systematic data for determining the accuracy of this guess are not readily available; however, there is sufficient evidence to confirm that volunteerism is far from being limited to a few Bay Area nonpartisan councils.

It has been noted already that the frequency with which incumbents are returned to office when they seek reelection is uniformly high for all types of legislatures in the United States. The rate of voter turnout is a more problematic variable because of the ambiguity of determining what is "high" and what is "low" turnout; however, both survey and aggregate data readily confirm that voter turnout fluctuates widely from one type of election to the next and, in the thousands upon thousands of elections below the state level, elections very often involve only small minorities of the eligible electorate. Data on appointment as a route to elective office are not available, but a reasonable guess is that the figure reported here (approximately one-fourth) does not greatly exaggerate the proportion of appointees to school boards, city and township councils, and so forth. The proportion of appointees to state legislatures and to the national congress should be much lower.

There are more systematic data on the frequency with which elected officials voluntarily depart from office, the remaining indicator of volunteerism. James Young's *The Washington Community—1800–1828,* is a good beginning point. Evidence reported by Young indicate that the federal congresses of the early decades should be placed well toward the "most volunteerism" end of a hypothetical continuum; indeed, the legislative body empowered to guide the nation in its early years had higher rates of voluntary departure than the councils presently governing the Bay Area cities. "From 1797 to 1829 (5th through 20th Congresses) more Senators resigned than failed to be reelected by their state legislatures." And, "Each new Congress, moreover, brought a host of new faces to the community, drastically reconstituting its membership every two years. For the first four decades of national government between one-third and two-thirds of the congressional community left every two years not to return."[22] On the average, the biennial turnover during the first decades of the founding was 41.5 per cent of the total legislative membership. Young's study is a prototype for investigating how voluntary turnover affects the governing process and especially the means by which accountability is insured. The burden of his analysis is that the legislators were "remote" from the citizens they were to govern and, in effect, that they were not held accountable for the policies they pursued.[23]

To move to more recent times, and from the national to the state level, it is instructive to review the data on voluntary retirement collected by Charles Hyneman three decades ago. He reports that much turnover among state legislators is due not to election defeat, but to voluntary retirement. In his extensive study of twenty-five state chambers between 1925 and 1935, he found that fewer than one-third of the retirements from state legislative office were due to election defeats; only 16 per cent came from general election defeats, the presumed sanction of the voting public. After his exhaustive study of turnover he commented, "The real task is to find why so many legislators, senators and representatives alike, choose not to run again."[24] Eulau, writing about state legislators some twenty years after the Hyneman study, reports similar figures. The proportion of legislators in each of four states who either intend not to stand for reelection or are doubtful about it is as follows: Tennessee—66 per cent; Ohio—40 per cent; California—34 per cent; and New Jersey—24 per cent.[25] Barber's study of Connecticut state legislators provides similar data. In Connecticut, "election defeat accounted for less than 26 per cent of retirement from the House in seven elections between 1946 and 1958. Thus, by far the largest proportion of retirements may be classified as pre-election turnover."[26] Among the first-term representatives interviewed by Barber, 35 per cent report that they definitely or probably would not be willing to serve more than two or three terms.

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[22] James S. Young, *The Washington Community—1800–1828* (New York: Columbia University Press, 1966), p. 57, 89.

[23] Young writes of the "remoteness of the rulers from the citizenry and remoteness of the citizenry from the rulers" and that the "isolated circumstances of the early governing group must have afforded a freedom of choice as nearly uninhibited as any representative government could have." *Ibid.*, p. 34, 36. Young, of course, is making much of the sheer fact of geographical isolation whereas volunteerism implies a distance between rulers and citizenry established not by geography but by the processes of leadership selection.

Additional confirmation that leadership turnover is often voluntary is to be found in data collected by Schlesinger. For twenty-nine states between 1914 and 1958, in only 17 per cent of 542 gubernatorial elections was the incumbent defeated. For thirty-six states during the same time period, in only one-fifth of the 616 senatorial elections was the incumbent defeated.[27] Although it is not possible to derive rates of voluntary retirement directly from these figures, Schlesinger presents other data which support the conclusion that rates of voluntary retirement are high even for governors and senators.[28]

This sketchy review suggests an hypothesis: there is an inverse relationship between volunteerism and perceived political stakes. "Important" legislatures have less volunteerism than "unimportant" ones. There are several variants on this theme. For instance, the higher the level of government, the fewer the number of voluntary departures from office. Another variant, the more professionalized (or better paid) the legislature, the lower the rate of volunteerism.[29] Further, the greater the competition to enter the legislature, the less volunteerism there is. And so on. Thus, whereas in the extent of volunteerism the national congresses of the 1800's may parallel nonpartisan city councils in the 1960's, the 80th Congress in no way resembles the 8th Congress.

Such qualifications, though important, do not negate the thesis developed in this paper. The thesis is theoretical—showing the relationship among variables pertinent to accountability theories, but also descriptive—discovering something about the likely distribution of these variables.

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[24] Charles S. Hyneman, "Tenure and Turnover of Legislative Personnel," *Annals of the American Academy of Political and Social Science,* Vol. 195 (1938), p. 20.

[25] John Wahlke, Heinz Eulau, William Buchanan, Leroy C. Ferguson, *The Legislative System* (New York: John Wiley & Sons, 1962), computed from Table 6.1, p. 122.

[26] David Barber, *The Lawmakers* (New Haven: Yale University Press, 1965), p. 8.

[27] Schlesinger, *op. cit.,* p. 63 (Table IV-2).

[28] This inference seems warranted on the basis of evidence Schlesinger presents on p. 144, regarding the frequency with which the candidates for gubernatorial and senatorial elections differ from their predecessors, and his statement on p. 146 that the opportunity for men to be nominated for these offices is "impressively high, indicative of a great deal of fluctuation at the very top of the pyramid within state parties." His data, of course, include controls for tenure limitations. There are two interpretations possible. Either the political parties habitually fail to renominate the incumbent, or the rates of voluntary retirement are high for governors and senators. The latter seems the more reasonable inference.

[29] Schlesinger writes that the "impact of ambitions upon the behavior of public officials will be greater on those in high than in low office, greater upon congressmen than upon state legislators, greater upon United States Senators than upon United States Representatives." *Ibid.,* p. 193. Although this observation is intuitively persuasive, it is relevant to record that it is an inference and in no way is it directly confirmed by the massive amounts of evidence Schlesinger imaginatively analyzes. Evidence suggesting that salary differentials may promote or retard political ambitions at the state level is suggested by John W. Soule, "Future Political Ambitions and the Behavior of Incumbent State Legislators," *Midwest Political Science Review,* XIII (August, 1969), pp. 443–444. For a study indicating that as a legislature becomes more professionalized over time, the incidence of volunteerism should drop, see Nelson Polsby, "The Institutionalization of the U. S. House of Representatives," this REVIEW, LXII (March, 1968), 144–168. To read Polsby's study side-by-side with Young's analysis of the early congresses is very instructive, and indicates why volunteerism as an attribute of a legislature is variable over time.

To review the thesis: the theory of electoral accountability predicts that elected officials keep in mind the preferences of ordinary citizens because these officials are determined to remain in office. This proposition has been confirmed in several empirical studies, and, in fact, is confirmed in Table 5. What is generally ignored by scholars associated with the accountability theory is (1) clearly establishing the conditions under which elections hold the representatives accountable, and (2) providing evidence regarding the distribution of these conditions. Our analysis speaks to both of these points: where the methods of selecting and displacing political leaders encourage the norm of volunteerism, electoral accountability is weakened; moreover, though undoubtedly more prevalent at the local than the state and at the state than the national level, the norm of volunteerism in some degree is present in most legislatures.

A postscript to this latter point is in order. The overwhelming majority of the half-million elected legislators in the United States serve in legislatures where some recognizable degree of volunteerism is present. The skeptic can say that these legislative bodies—city councils, school boards, village governments, etc.—are "unimportant" because they deal with "low-stake issues." While true that the House Committee on Education and Labor is far more important to the education of American citizens than any single school board, the cumulative impact of nearly 35,000 school boards is in turn far greater than the action of a Congressional Committee. Thus, whereas there is one national legislature and 50 state legislatures (where already evidence of volunteerism is apparent), there are in excess of 3,000 county governments, in excess of 35,000 municipalities, towns, and townships, nearly 35,000 school boards, as well as 18,000 special districts.

I do not deny the symbolic as well as material importance of the central government, yet if the ordinary citizen is subject at the local level to legislatures seemingly remote and indifferent, that citizen's "experience with democracy" falls short not only of the classical model but also of the neo-classical model which emphasizes electoral accountability. It is in this context that we should interpret survey results such as, for instance, McClosky's finding that approximately half of the general electorate "perceive government and politicians as remote, inaccessible, and largely unresponsive to the electorate's needs or opinions."[30] Volunteerism among elected officials may well be connected with political disillusionment among the electors.

## V. SPECULATIONS AND CONCLUSIONS

Perhaps the most efficient manner in which to summarize the thesis and findings presented here is to organize the conclusions around a few general speculations. Speculations permit us to review prominent themes in writings about electoral accountability, from the viewpoint of findings about volunteerism, and yet avoid the dangers of premature precision.

*Leadership Turnover, Accountability, and Volunteerism.* To consider democratic politics from the perspective of electoral accountability theories has, for many writers, implied sharing with Lipset the belief that permanent insecurity for those in governing positions makes for democratic politics. When leaders are entrenched they form an oligarchy; and oligarchies are inimical to political processes which would insure some fit between the preferences of non-leaders and the policies of leaders. Insecure tenure, in contrast, assures that leaders will attempt to retain their high status positions by satisfying the policy demands of at least some sizable part of the citizenry.

This formulation seems reasonable until we encounter volunteerism in the methods of leadership selection. When movement into and out of office is self-determined, rapid turnover of elected leaders, far from being an indicator of insecurity and, hence accountability, is more likely related to an indifference among the elected to the sanctions of the ballot box. Indeed, when we discovered in the city council study that leadership turnover was not necessarily linked to election defeats, we were led to distinguish between the "circulation of elites" and the "replaceability of elites." The former term implies that leadership turnover is associated with changes in the mood of the citizens and thus with changes in policies; the latter term makes no such assumptions. Where there is recruitment by replaceability, a procession of like-minded men move through public office with relatively little resultant shift in policies.

*Civic Duty, Social Status, and Malrepresentation of Social Groups.* The inquiry into volunteerism also encourages some revised thinking about a topic closely related to the issue of tenure insecurity. There is in the study of democratic politics considerable attention to the topic of social class and political recruitment. One perspective on this question treats the political processes which accentuate and those which depress the tendency for the members of the relatively better-off social groups in society to be significantly overrepresented in political leadership circles.

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[30] Herbert McClosky, "Consensus and Ideology in American Politics," this Review, LVIII, (June, 1964), 361–382. For instance, as reported on pp. 370–371, more than half the general electorate agree that "It seems to me that whoever you vote for, things go on pretty much the same" and "There is practically no connection between what a politician says and what he will do once he gets elected," and "Nothing I ever do seems to have any effect upon what happens in politics."

Where electoral accountability is fully operating, this tendency should be depressed. Candidates for office must appeal to a relatively wide range of social groups in order to reach and to hold political office. Where volunteerism affects leadership selection, class bias in the composition of the legislature is likely to be accentuated. The reasoning here is simple.

First, the smallness of the voting constituency increases the probability that the constituency is socially homogeneous, and, based on what we know about the correlates of voting behavior, consequently increases the probability that the constituency is middle and upper-middle class.[31] Second, a more complicated but possibly more critical factor relates to the motives for seeking office. When the selection of leaders is characterized by volunteerism, the recruitment processes necessarily further careers of the self-selected. For the cities we study this implies that the pool of eligible recruits for city office are those citizens with a strong sense of civic duty. That the positions are nonsalaried adds to this tendency. Although the term "civic duty" can imply many things, among the citizens who govern cities and towns, school boards and special districts, and so forth, it very often implies a belief that one has the duty to direct the political community.[32] This belief is never randomly distributed across all socio-economic groupings in society. Political socialization studies show that white middle and upper-middle class children, more so than children from less well-to-do social groups, are taught to believe in their "duties" as citizens and their "responsibilities" as community leaders. Civic duty tends to be little more than a middle-class version of *noblesse oblige;* it is not nobility status which obligates but bourgeois status. Political volunteerism derives from citizen duty, and malrepresentation of social groups in legislatures is one consequence.

*Status Inequality, Ambitions, and Volunteerism.* The investigation of volunteerism might make a contribution to a related inquiry which attempts to link accountability to issues of social status. Lipset, elaborating on Michels, notes that democratic accountability is threatened when the status difference between political leaders and political followers grows too great.[33] If there is great status inequality, those in office will have cause to resist leadership turnover and, moreover, will have the superior resources it requires to forestall challenges from an opposition. The difficulties of establishing constitutional democracy in several African nations lends support to this hypothesis.

A contrary position has been advanced by May[34] and, indirectly, by Schlesinger. When political office confers no status, then the motive for seeking office is not political ambition but, for instance, civic duty. Schlesinger reminds us that the absence of ambition in politics can be inimical to representative democracy: "No more irresponsible government is imaginable than one of high-minded men unconcerned for their political futures."[35] The earlier cited responses of elected councilmen lends support to this point; as one councilman stated it, "I am free do do as I feel. In general it is easy to vote against the majority because I don't have any political ambitions."

The data on volunteerism helps clear up the contradiction between the "status inequality" hypothesis and the "ambition" hypothesis. The question is not whether political leaders have higher status for, as Eulau reminds us, this is almost certainly the case.[36] At issue is not the status distance, but whether office confers status or whether office is simply a by-product of previously achieved status, which is to say previously acquired prestige, wealth, leisure, security, and so forth. Where political office is but a by-product, the conditions leading to volunteerism will be accentuated. Electoral accountability can be expected to suffer.

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[31] An interesting study which shows that the socio-economic composition of legislatures is related to voter turnout is that of Rosalio Wences, "Electoral Participation and the Occupational Composition of Cabinets and Parliaments," *American Journal of Sociology,* vol. 75 (September, 1969), pp. 181–192. A related discussion is in Kenneth Prewitt, "From the Many Are Selected the Few," *American Behavioral Scientists,* November, 1969.

[32] A finding of McClosky's is instructive in this context. One of the very few items on which political influentials measured higher than the general electorate was as follows: "Most politicians can be trusted to do what they think is best for the country." Seventy-seven percent of the influentials agreed and fifty-nine percent of the electorate did. *op. cit.,* p. 370.

[33] Lipset, et al., *op. cit.,* especially pp. 239–247.

[34] A paper by John D. May, unfortunately not yet published, is particularly insightful on this issue. See his "Democracy and Inequality," unpublished paper, Chicago, 1969.

[35] Schlesinger, *op. cit.,* p. 2.

[36] Heinz Eulau, "Changing Views of Representation," in Ithiel de Sola Pool (ed) *Contemporary Political Science* (New York: McGraw-Hill, Inc., 1967), p. 80.

*Volunteerism and the Political Formula.* Inquiry into political representation, following Burke's lead, has established that representatives define their relationship with constituencies according to a trustee role-orientation, a delegate role-orientation, or, possibly, some mixture.[37] Available evidence testifies to the widespread presence of the trustee orientation among legislators in the United States; and though theories of electoral accountability have never been joined with studies of representational role-orientations, it is a reasonable speculation that trusteeship and volunteerism can comfortably coexist in the same legislative system.[38] Moreover, volunteerism and trusteeship are congenial to an interpretation of elections which stresses the ceremonial and ritualistic function they perform.

To suggest that volunteerism in political recruitment, trusteeship in political representation, and ritualism in political elections form a compatible package does not mean that these political phenomena necessarily coexist. But that they might coexist suggests an entirely new perspective on the theory of electoral accountability. Mosca will help us to sharpen this perspective.

For Mosca, the power of any political class rests upon a political formula or the set of moral principles which justifies the political hierarchy to those toward its bottom as well as those toward its top. Mosca describes the political formula in the United States: "The powers of all lawmakers, magistrates and governmental officials in the United States emanate directly or indirectly from the vote of the voters, which is held to be the expression of the sovereign will of the whole American people."[39] That Mosca himself considered this belief to be myth does not make it so. That he was correct in asserting that the vast majority of Americans believe in the formula, however, cannot be denied. One need only consult a portion of the data stored in political socialization studies, survey data, civics textbooks, political histories, journalistic imagery, and campaign speeches to conclude, with Mosca, that political leaders in this society are granted legitimacy because they are elected.

It has been the accomplishment of theorists working with electoral accountability notions to effect a subtle translation of Mosca's observation. Mosca's insight into the process which *legitimates* political leaders has been converted into a statement about the process which holds them *accountable*. But this is to confuse what should be separate issues. The electors and the elected can share a belief in the legitimacy of the political hierarchy because votes are cast and counted, without that same counting of ballots necessarily leading to accountability.

The operative political formula as described by Mosca is considered to be congenial to electoral accountability; less often noted is that the political formula is equally congenial to a politics where accountability does not proceed from electoral sanctions. Volunteerism in political recruitment, trusteeship in political representation, and ritualism in elections provide appropriate conditions. Political leaders are legitimated without feeling accountable. This would be so if, first, the norms shared by political leaders stress mutual responsibility for the public interest (trusteeship) without at the same time stressing accountability; such norms bring leaders closer together while simultaneously widening the distance between any given set of leaders and the constituency it represents.[40] And if, second, the political norms widely shared by citizens are such as to temper direct and regular involvement in political activities; that such is the case in the United States is the burden of much survey data.[41]

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[37] Wahlke, Eulau, *et al., op. cit.*, is the most elaborate application of the Burkean categories to the empirical study of legislators.

[38] A measure of a council attribute related to trusteeship is something we term the "city father ethos;" a term which describes an orientation of paternalistic responsibility among the councilmen. Where volunteerism was most present, 65% of the councils are characterized by this orientation; where volunteerism is least present, 40% of the councils are characterized by the orientation. A great deal of additional analysis remains to be completed, however, before we can confirm that volunteerism and trusteeship are empirically linked.

[39] Gaetano Mosca, *The Ruling Class* (New York: McGraw-Hill, Inc., 1939), p. 70.

[40] I have paraphrased the argument advanced in Parry, *op cit.*, in this sentence. It has been the substantial and brilliant contribution of Theodore Lowi, in his *The End of Liberalism* (New York: W. W. Norton & Co., Inc., 1969), to document exactly this tendency in contemporary politics. See, for instance, chapter 3, where he relates his thesis to the problem of holding leaders accountable for their actions.

[41] Gabriel Almond and Sidney Verba, *The Civic Culture* (Princeton: Princeton University Press,

Two points remain to be made. First, the speculation advanced in these last few paragraphs refers to electoral accountability and not to whether leaders consult the preferences of non-leaders. The question of consultation is separate from the issue of accountability. The point we stress is that if the consultative process occurs to any significant extent where volunteerism is present, it is for reasons other than an accountability forced by electoral sanctions.

Second, the last few decades of empirical research have seen an emphasis on electoral accountability as the process which links the few who govern and the many who are governed. The study of politics has been advanced by this insight. But if the theory of electoral accountability has added to an understanding of democratic politics, it has also distracted attention from political processes which weaken electoral sanctions. Volunteerism is one of these processes. Volunteerism as well as other political processes which weaken electoral sanctions are undoubtedly present in the strikingly large number of legislative systems in the United States. There are very many small groups of citizens elected to make laws for the greater number of citizens. It is a research task of practical as well as theoretical significance, to discover what, if anything, establishes democratic controls over legislatures somewhat immune from electoral sanctions.

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1963), especially pp. 476–487. The authors actually refer to their finding as constituting a myth. The citizen's perception that he can be influential "may be in part a myth, for it involves a set of norms of participation and perceptions of ability to influence that are not quite matched by actual political behavior," p. 481.

# PRESIDENTIAL POPULARITY FROM TRUMAN TO JOHNSON[1]

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> I think [my grandchildren] will be proud of two things. What I did for the Negro and seeing it through in Vietnam for all of Asia. The Negro cost me 15 points in the polls and Vietnam cost me 20.
>
> Lyndon B. Johnson[2]

With tenacious regularity over the last two and a half decades the Gallup Poll has posed to its cross-section samples of the American public the following query, "Do you approve or disapprove of the way (the incumbent) is handling his job as President?" The responses to this curious question form an index known as "Presidential popularity." According to Richard Neustadt, the index is "widely taken to approximate reality" in Washington and reports about its behavior are "very widely read"[3] there, including, the quotation above would suggest, the highest circles.

Plotted over time, the index forms probably the longest continuous trend line in polling history. This study seeks to analyze the behavior of this line for the period from the beginning of the Truman administration in 1945 to the end of the Johnson administration in January 1969 during which time the popularity question was asked some 300 times.[4]

Four variables are used as predictors of a President's popularity. These include a measure of the length of time the incumbent has been in office as well as variables which attempt to estimate the influence on his rating of major international events, economic slump and war. To assess the independent impact of each of these variables as they interact in association with Presidential popularity, multiple regression analysis is used as the basic analytic technique.

## I. THE DEPENDENT VARIABLE: PRESIDENTIAL POPULARITY

The Presidential popularity question taps a general impression about the way the incumbent seems to be handling his job at the present moment. As Neustadt notes, the response, like the question, is "unfocused,"[5] unrelated to specific issues or electoral outcomes. The respondent is asked to "approve" or "disapprove" and if he has "no opinion," he must volunteer that response himself. He has infrequently been asked *why* he feels that way—and many respondents when asked are able only vaguely to rationalize their position.[6] And only at times has he been asked to register how strongly he approves or disapproves.

A disapproving response might be considered a non-constructive vote of no-confidence: the respondent registers his discontent, but he does not need to state who he would prefer in the Presidency. Thus the index is likely to be a very imperfect indicator of success or failure for a President seeking re-election. While approvers are doubtless more likely than disapprovers to endorse his re-election, on considering the opposition some approvers may be attracted into voting against the incumbent just as some disapprovers may be led grudgingly to vote for him.[7]

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[1] This investigation was supported by a grant from the National Science Foundation. At various stages helpful comments, criticisms, and complaints were lodged by Richard Fenno, Gerald Kramer, Richard Niemi, Peter Ordeshook, Alvin Rabushka, William Riker, and Andrew Scott.

[2] Quoted, David Wise, "The Twilight of a President," *New York Times Magazine*, November 3, 1968, p. 131.

[3] Richard E. Neustadt, *Presidential Power: The Politics of Leadership* (New York: Wiley, 1960), p. 205n.

[4] A general picture of what this line looks like can be gained from the figure in Robert A. Dahl, *Pluralist Democracy in the United States* (Chicago: Rand McNally, 1967), p. 107. The Presidential popularity data for the Johnson administration have been taken from the *Gallup Opinion Index*. All other poll data, unless otherwise indicated, have come from the archives of the Roper Public Opinion Research Center at Williams College, Williamstown, Massachusetts.

[5] *Op. cit.*, p. 96.

[6] See for example the breakdowns in *Gallup Opinion Index*, March, 1966, p. 4.

[7] There is also a more technical reason why the popularity index has little direct relevance to the electoral result: Gallup does not ask the question during a President's re-election campaign. Thus for the months between early summer and late fall in 1948, 1956, and 1964 no Gallup data on Presidential popularity exist. One other technical-

Whatever peculiarities there are in the question itself, they are at least constant. Unlike many questions asked by the polling organizations, wording has not varied from time to time by whim or fashion. The stimulus has therefore been essentially fixed; only the response has varied.

And the variation has been considerable. Harry Truman was our most popular President in this period—for a few weeks in 1945 when more than 85 percent of the public expressed approval—and our least popular—from early 1951 until March 1952 when less than 30 percent were usually found to be favorably inclined. Other Presidents have stayed within these limits with Lyndon Johnson most nearly approaching the Truman extremes. President Eisenhower's popularity was never higher than 79 percent, but it never dropped below 49 percent either. President Kennedy also maintained a rather high level of popularity but was in noticeable decline at the time of his death.

The proportion of respondents selecting the "no opinion" option, averaging 14 percent, remained strikingly constant throughout the period.[8] This is a little surprising since it might be expected that when opinion changes, say, from approval to disapproval of a President, the move would be seen first in a decrease in the support figure with an increase in the no opinion percentage, followed in a later survey by an increase in the disapproval column with a decrease in the no opinion portion. There are a few occasions in which the no opinion percentage seems to rise and fall in this manner, one occurring in the early weeks of the Korean War, but by and large it would appear that if movements into the no opinion column do occur they are compensated for by movements out of it.

This means therefore that the trend in approval is largely a mirror image of the trend in disapproval; the correlation between the two is −.98. And, most conveniently, this almost means that the President's popularity at a given moment can be rendered by a single number: the percentage approving his handling of the job. The no opinion percentage is almost always close to 14 percent and the percentage disapproving is, of course, the remainder.

There is, however, one small wrinkle. The no opinion percentage does get a bit out of hand, quite understandably, in the early weeks of the Kennedy and Eisenhower administrations as substantial numbers of respondents felt inclined to withhold judgment on these new men. This inordinate withholding of opinion declined in the first weeks to more "normal" levels with the result that *both* the level of approval *and* disapproval tended to increase.[9]

Since one of the propositions to be tested in this study proposes that there exists a general downward trend in each President's popularity, this initial rating situation causes something of a problem. If the disapproval score is used as the dependent variable there will be a slight bias in favor of the proposition. It seems preferable to load things against the proposition; hence for the purposes of this study *the dependent variable is the percentage approving the way the incumbent is handling his job as President.*[10] The average approval rating for the entire twenty-four year period is 58 percent.

### II. THE INDEPENDENT VARIABLES

If one stares at Presidential popularity trend lines long enough, one soon comes to imagine one is seeing things. If the things imagined seem also to be mentioned in the literature about the way Presidential popularity should or does behave, one begins to take the visions seriously and to move to test them.

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ity is worth mention. There is a slight underrepresentation of data points in Truman's first years. By 1950, except for the election year phenomenon, the Gallup organization was asking the question on virtually every survey conducted—some dozen or sixteen per year. Before that time the question was posed on the average only about half as frequently. Neither of these technical problems, however, is likely to bias the results in any important way, especially since so much of the analysis allows each administration a fair amount of distinctiveness.

[8] The standard deviation for the no opinion response is 2.93. By contrast the comparable statistic is 14.8 for the approve response and 14.5 for the disapprove response.

[9] In the case of President Eisenhower, the no opinion response actually rose a bit before it began to descend, reaching the highest level recorded for any President in the period in March 1953 when 28 percent had no opinion. (President Nixon has proved to be the greatest mystery of all: fully 36 percent registered no opinion after his inauguration.)

[10] It is argued by some that percentages should not be used in their pure state as variables, but rather should be transformed into logits: $Y^* = \log_e [Y/(1-Y)]$. The transformation was tried in the analysis, but it made little difference. Therefore the more easily communicated percentage version has been kept. In any event the dependent variable rarely takes extreme values. It rises to 80 percent only three or four times and never dips below 23 percent.

In this manner were formulated four basic "independent" variables, predictor variables of Presidential popularity. They are: 1) a "coalition of minorities" variable that suggests the overall trend in a President's popularity will be downward; 2) a "rally round the flag" variable which anticipates that international crises and similar phenomena will give a President a short-term boost in popularity; 3) an "economic slump" variable associating recessions with decreased popularity; and 4) a "war" variable predicting a decrease in popularity under the conditions of the Korean and Vietnam wars.

1. *The "coalition of minorities" variable.* In a somewhat different context Anthony Downs has suggested the possibility that an administration, even if it always acts with majority support on each issue, can gradually alienate enough minorities to be defeated. This could occur when the minority on each issue feels so intensely about its loss that it is unable to be placated by administration support on other policies it favors. A clever opposition, under appropriate circumstances, could therefore forge a coalition of these intense minorities until it had enough votes to overthrow the incumbent.[11]

Transposed to Presidential popularity, this concept might inspire the expectation that a President's popularity would show a general downward trend as he is forced on a variety of issues to act and thus create intense, unforgiving opponents of former supporters. It is quite easy to point to cases where this may have occurred. President Kennedy's rather dramatic efforts to force back a steel price rise in 1962, while supported by most Americans, tended to alienate many in the business community.[12] Administration enforcement of the Supreme Court's school desegregation order tended to create intense opposition among white Southerners even if the Presidential moves had passive majority support in most of the country.[13]

Realistically, the concept can be extended somewhat. From time to time there arise exquisite dilemmas in which the President must act and in which he will tend to alienate *both* sides no matter what he does, a phenomenon related to what Aaron Wildavsky has called a "minus sum" game.[14] President Truman's seizure of the steel mills in 1952 made neither labor nor management (nor the Supreme Court, for that matter) happy. For the mayor of New York, situations like this seem to arise weekly.

There are other, only vaguely related, reasons to expect an overall decline in popularity. One would be disillusionment. In the process of being elected, the President invariably says or implies he will do more than he can do and some disaffection of once bemused supporters is all but inevitable. A most notable example would be the case of those who supported President Johnson in 1964 because he seemed opposed to escalation in Vietnam. Furthermore initial popularity ratings are puffed up by a variety of weak followers. These might include leering opposition partisans looking for the first excuse to join the aggrieved, excitable types who soon became bored by the humdrum of post-election existence, and bandwagon riders whose fair weather support dissolves with the first sprinkle.[15] As Burns Roper notes, "In a sense, Presidential elections are quadriennial myth builders which every four years make voters believe some man is better than he is. The President takes office with most of the nation on his side, but this artificial 'unity' soon begins to evaporate."[16]

For these reasons the coalition of minorities variable, as it is dubbed here, predicts decline. "Love," said Machiavelli, "is held by a chain of obligation which, men being selfish, is broken whenever it serves their purpose."[17]

*The coalition of minorities variable is measured simply by the length of time, in years, since the incumbent was inaugurated (for first terms) or re-elected (for second terms).* It varies then from zero to about four and should be negatively correlated with popularity: the longer the man has been in office, the lower his popularity. It is; the simple r is −.47. The decline is assumed to start over again for second terms because the President is expected to have spent the campaign rebuilding his popular coalition by soothing the disaffected, re-deluding the disillusioned, and putting on a show for the bored. If he is unable to do this, he will not be re-elected, something which has not happened in the post-war era although twice Presidents have declined to make the effort.

The analysis will assume a *linear* decline in popularity. That is, a President's popularity is assumed to decline at an even rate for all four years of his term: if a decline of 6 percentage points per year is indicated, he will be down 6 points at the end of his first year, 12 at the end of the second, 18 at the end of the third, and 24 after four years. There is nothing in the justification for the coalition of minorities variable which demands that the decline must occur with such tedious regularity but, when curvilinear variants were experimented with, little or no improvement was found. Hence the reliance in this study on the linear version which has the advantage of simplicity and ease of communication.

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[11] *An Economic Theory of Democracy* (New York: Harper and Row, 1957), pp. 55–60.

[12] See the data in H. G. Erskine, "The Polls," 28 *Public Opinion Quarterly*, 341 and 338 (Summer 1964).

[13] See John M. Fenton, *In Your Opinion* (Boston: Little, Brown, 1960), p. 146.

[14] "The Empty-head Blues: Black Rebellion and White Reaction," *The Public Interest*, Spring 1968, pp. 3–16.

[15] On the bandwagon effect among nonvoters, see Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter* (New York: Wiley, 1960), pp. 110–115.

[16] Burns Roper, "The Public Looks at Presidents," *The Public Pulse*, January 1969.

[17] *The Prince*, ch. XVII.

2. *The "rally round the flag" variable.* This variable seeks to bring into the analysis a phenomenon often noted by students of the Presidency and of public opinion: certain intense international events generate a "rally round the flag" effect which tends to give a boost to the President's popularity rating. As Kenneth Waltz has observed, "In the face of such an event, the people rally behind their chief executive."[18] Tom Wicker: "Simply being President through a great crisis or a big event . . . draws Americans together in his support."[19] Richard Neustadt notes "the correspondence between popularity and happenings,"[20] Burns Roper finds "approval has usually risen during international crises,"[21] and Nelson Polsby observes, "Invariably, the popular response to a President during international crisis is favorable, regardless of the wisdom of the policies he pursues."[22]

The difficulty with this concept is in operationalizing it. There is a terrible temptation to find a bump on a popularity plot, then to scurry to historical records to find an international "rally point" to associate with it. This process all but guarantees that the variable will prove significant.

The strategy adopted here to identify rally points was somewhat different and hopefully more objective. A definition of what a rally point should look like was created largely on *a priori* grounds and then a search of historical records was made to find events which fit the definition. Most of the points so identified *are* associated with bumps on the plot—that after all was how the concept was thought of in the first place—but quite a few are not and the bumps associated with some are considerably more obvious than others.

In general, a rally point must be associated with an event which 1) is international and 2) involves the United States and particularly the President directly; and it must be 3) specific, dramatic, and sharply focused.

It must be international because only developments confronting the nation as a whole are likely to generate a rally round the flag effect. Major domestic events—riots, scandals, strikes —are at least as likely to exacerbate internal divisions as they are to soothe them.

To qualify as a rally point an international event is required to involve the United States and the President directly because major conflicts between other powers are likely to engender split loyalties and are less likely to seem relevant to the average American.

Finally the event must be specific, dramatic and sharply focused in order to assure public attention and interest. As part of this, events which transpire gradually, no matter how important, are excluded from consideration because their impact on public attitudes is likely to be diffused. Thus sudden changes in the bombing levels in Vietnam are expected to create a reaction while the gradual increase of American troops is not.

Errors in this process could occur by including events whose importance is only obvious in retrospect or by ignoring events like the Geneva summit of 1955 which may seem minor in historical perspective but were held significant at the time. For this reason more reliance has been put on indexes of newspaper content than on broad, historical accounts of the period.[23] In general if there has been a bias in selecting rally points it has been in the direction of excluding border-line cases. This was done in profound respect for the lack of public interest and knowledge on most items of international affairs.

At that, some 34 rally points were designated. In general they can be said to fall into six categories. First, there are the four instances of sudden American military intervention: Korea, Lebanon, the Bay of Pigs, and the Dominican Republic. A second closely related category encompasses major military developments in ongoing wars: in Korea, the Inchon landing and the Chinese intervention; in Vietnam, the Tonkin

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[18] Kenneth N. Waltz, "Electoral Punishment and Foreign Policy Crisis" in James N. Rosenau, *Domestic Sources of Foreign Policy* (New York: Free Press, 1967), p. 272.

[19] Tom Wicker, "In the Nation: Peace, It's Wonderful," *New York Times,* July 4, 1967, p. 18.

[20] *Op. cit.,* p. 100.

[21] *Op. cit.*

[22] *Congress and the Presidency* (Englewood Cliffs, N. J.: Prentice-Hall, 1964), p. 25.

[23] Especially valuable was Eric V. Nordheim and Pamela B. Wilcox, "Major Events of the Nuclear Age: A Chronology to Assist in the Analysis of American Public Opinion," Oak Ridge National Laboratory, Oak Ridge, Tennessee, August 1967. Other sources often consulted included the *New York Times Index* and the Chronology section of the *World Almanac.*

Bay episode, the beginning of bombing of North Vietnam, the major extension of this bombing, and the Tet offensive. Third are the major diplomatic developments of the period: crises over Cuban missiles, the U-2 and atomic testing, the enunciation of the "Truman doctrine" with its offer of aid to Greece and Turkey, the beginning of, and major changes in, the peace talks in Korea and Vietnam, and the several crises in Berlin. Fourth are the two dramatic technological developments: Sputnik and the announcement of the first Soviet atomic test. The fifth category includes the meetings between the President and the head of the Soviet Union at Potsdam in 1945, Geneva in 1955, Camp David in 1959, Paris in 1960, Vienna in 1961, and Glassboro in 1967. While these events are rarely spectacular they, like crisis, do generate a let's-get-behind-the-President effect. Because they are far less dramatic—even if sometimes more important—Presidential conferences with other powers (e.g., the British at Nassau) are excluded as are American meetings with the Soviet Union at the foreign minister level.

Sixth and finally, as an analytic convenience the start of each Presidential term is rather arbitrarily designated as a rally point. Presidents Truman and Johnson came in under circumstances which could justifiably be classified under the "rally round the flag" rubric although the crisis was a domestic one. The other points all involve elections or re-elections which perhaps might also be viewed as a somewhat unifying and cathartic experience.

These then are the events chosen to be associated with the rally round the flag variable. No listing will satisfy everyone's perspective about what has or has not been important to Americans in this 24 year period. However, in the analysis the variable has proven to be a rather hardy one. Experimentation with it suggests the addition or subtraction of a few rally points is likely to make little difference.

*The rally round the flag variable is measured by the length of time, in years, since the last rally point.* It varies then from zero to a theoretical maximum of about four or an empirical one of 1.9. Like the coalition of minorities variable, it should be negatively correlated with popularity: the longer it has been since the last rally round the flag event, the lower the popularity of the incumbent. It is; the simple r is −.11. Some experiments with curvilinear transformations of the variable were attempted but, since improvement again was marginal at best, the variable has been left in linear form.

Each rally point is given the same weighting in the analysis. One effort to soften this rather crude policy was made. The rally points were separated into two groups: "good" rally points (e.g., the Cuban missile crisis) in which the lasting effect on opinion was likely to be favorable to the President and "bad" one (e.g., the U-2 crisis, the Bay of Pigs) in which the initial favorable surge could be expected to be rather transitory. Two separate rally round the flag variables were then created with the anticipation that they would generate somewhat different regression coefficients. The differences however were small and inconsistent. The public seems to react to "good" and "bad" international events in about the same way. Thus, to this limited extent, the equal weighting of rally points seems justified.

In tandem, the concepts underlying the coalition of minorities and rally round the flag variables predict that the President's popularity will continually decline over time and that international crises and similar events will explain short term bumps and wiggles in this otherwise inexorable descent.[24]

3. *The "economic slump" variable.* There is a goodly amount of evidence, and an even goodlier amount of speculation, suggesting a relationship between economic conditions and electoral behavior. The extension of such thinking to Presidential popularity is both natural and precedented. Neustadt, for example, concludes the recession in 1958 caused a drop in President Eisenhower's popularity.[25]

The economic indicator used here will be the unemployment rate. The statistic recommends itself because it is available for the entire period and is reported on a monthly basis.[26] It is used as a general indicator of economic health or malaise and is not taken simply as a comment about the employed. It is assumed that the individual respondent, in allowing economic perceptions to influence him, essentially does so by comparing how things are *now* with how they were when the incumbent began his present term of office. If conditions are worse, he is in-

[24] Roper notes that President Kennedy's highest point of popularity occurred after the Bay of Pigs invasion and concludes this fact says something special about that crisis event *(op. cit.)* But this phenomenon is due to *two* effects—the rally round the flag effect *and* the fact that the event occurred very early in Kennedy's administration when the value for the coalition of minorities variable was yet very low.

[25] *Op. cit.*, p. 97ff.

[26] Data were gathered from Geoffrey H. Moore (ed.), *Business Cycle Indicators*, Vol. II (Princeton: Princeton University Press, 1961), p. 122; and, for more recent data, issues of the *Monthly Labor Review*.

clined to disapprove the President's handling of his job, if things are better he is inclined to approve. The economic variable therefore becomes the unemployment rate at the time the incumbent's term began subtracted from the rate at the time of the poll.[27] It is positive when things are worse and negative when things are better. It should be negatively correlated with popularity. But it isn't.

Unemployment reached some of its highest points during the recessions under the Eisenhower administration. The problem, to be examined more fully in Section VI below, is that Eisenhower was a *generally* popular President. Thus even though his popularity seemed to dip during the recessions, high unemployment comes to be associated with a relatively popular President. This problem can be handled rather easily within regression analysis by assigning to each of the Presidential administrations a "dummy" variable, the care and feeding of which will be discussed more fully in Section III below.

However, even when this circumstance is taken into account, the correlation coefficient and the regression coefficient for the economic variable remain positive. This seems to be largely due to the fact that *both* unemployment and the popularity of the incumbent President were in general decline between 1961 and 1968. The correlation for the period is .77.

Therefore a final alteration administered to the economic variable was to set it equal to zero whenever the unemployment rate was lower at the time of the survey than it had been at the start of the incumbent's present term. This alteration is a substantive one and is executed as the only way the data can be made to come out "right." In essence it suggests that *an economy in slump harms a President's popularity, but an economy which is improving does not seem to help his rating.* Bust is bad for him but boom is not particularly good. There is punishment but never reward.

Perhaps this can be seen in a comparison of the 1960 and the 1968 campaigns. In 1960, as Harvey Segal notes, "What was important was the vague but pervasive feeling of dissatisfaction with the performance of the economy, the pain that made the public receptive to JFK's appeals."[28] In 1968, representing administrations that had presided over an unprecedented period of boom, Vice President Humphrey never seemed able to turn this fact to his advantage.

It is important to note that in practice this variable, which will be called the "economic slump" variable because of its inability to credit boom, takes on a non-zero value only during the Eisenhower administration and during the unemployment rise of 1949–50. In symbolic form the variable's peculiarities can be expressed in the following; the units are the percentage of unemployed:

$$\begin{aligned} E &= U_t - U_{t_0} && \text{if } U_t - U_{t_0} > 0 \\ &= 0 && \text{if } U_t - U_{t_0} \leq 0 \end{aligned}$$

where

$U_t$ = Unemployment rate at the time of the survey

and

$U_{t_0}$ = Unemployment rate at the beginning of the incumbent's present term

4. *The "war" variable.* It is widely held that the unpopular, puzzling, indecisive wars in Korea and Vietnam severely hurt the popularity of Presidents Truman and Johnson.[29] As noted in the quotation that heads this report, President Johnson himself apportions 20 percentage points of his drop in popularity to the Vietnam War.

This notion seems highly plausible. The popularity of Presidents Truman and Johnson was in steady decline as the wars progressed with record lows occurring during each President's last year in office at points when the wars seemed most hopeless and meaningless. The wars unquestionably contributed in a major way to their decisions not to seek third terms and then, when they had stepped aside, the wars proved to be major liabilities for their party's candidates in the next elections. Overall, the correlation between Presidential popularity and the presence of war is −.66.

There are problems with this analysis, however. The coalition of minorities concept argues that decline is a natural phenomenon and, indeed, a glance at a plot of Presidential popularity clearly shows Truman and Johnson in decline *before* the wars started. Furthermore, as will be seen, both men experienced noticeable declines during their *first* terms when they had no war to contend with. The real question is, then, did the war somehow add to the decline of popularity beyond that which might be expected to occur on other grounds?

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[27] One wrinkle, which is intuitively comfortable but makes little difference in the actual results, was to do something about the unemployment rates at the start of the first terms of Presidents Truman and Eisenhower when unemployment was "artifically" depressed due to ongoing wars. Presumably the public would be understanding about the immediate postwar rise in unemployment. Therefore for these two terms the initial unemployment level was taken to be that level which held six months after the war ended while the economic variable for the few months of the war and the six month period was set equal to zero.

[28] "The Pain Threshold of Economics in an Election Year," *New York Times*, July 15, 1968.

[29] Waltz, *op. cit.*, pp. 273ff, 288; Neustadt, *op. cit.*, pp. 97–99; Wicker, *op. cit.*; Roper, *op. cit.*

An answer can be approached through multiple regression analysis. After allowing for a general pattern of decline under the coalition of minorities variable, the additional impact of a variable chosen to represent war can be assessed. It is also possible in this manner to compare the two wars to see if their association with Presidential popularity differed.

*The presence of war is incorporated in the analysis simply by a dummy variable that takes on a value of one when a war is on and remains zero otherwise*. The beginning of the Vietnam War was taken to be June 1965 with the beginnings of the major US troop involvement. At that point it became an American war for the public; before that ignorance of the war was considerable: as late as mid-1964, 25 percent of the public admitted it had never heard of the fighting in Vietnam.[30]

Other war measures of a more complex nature were experimented with. They increase in magnitude as the war progresses and thus should be able to tap a wearying effect as the years go by and should be negatively associated with popularity. These measures, however, are very highly correlated with the coalition of minorities variable for the two relevant Presidential terms and thus are all but useless in the analysis. The simple dummy variable suffers this defect in lesser measure, although it is far from immune, and thus, despite its crudities, has been used.[31]

5. *Other variables*. The analysis of Presidential popularity will apply in various ways only the four variables discussed above—a rather austere representation of a presumably complex process. As will be seen, it is quite possible to get a sound fit with these four variables, but at various stages in the investigation—which involved the examination of hundreds of regression equations—a search was made for other variables which could profitably be added to the predictor set.

International developments are reasonably well incorporated into the analysis with a specific variable included for war and another for major crisis-like activities. Domestically, however, there is only the half-time variable for economic slump and the important but very unspecific coalition of minorities variable.

Accordingly it would be valuable to generate some sort of domestic equivalent to the rally round the flag variable to assess more precisely how major domestic events affect Presidential popularity. Operationally, however, this is a difficult task. First, while it is a justifiable assertion that international crises will redound in the short term to a President's benefit it is by no means clear how a domestic crisis, whether riot, strike, or scandal, should affect his popularity. Furthermore major domestic concerns have varied quite widely not only in intensity and duration, but also in nature. Labor relations, which rarely made big news in the mid-1960s, were of profound concern in the middle and late 1940s as a multitude of major strikes threatened to cripple the nation and the adventures of John L. Lewis and the Taft-Hartley bill dominated the headlines. In the 1950s, however, labor broke into the news only with an occasional steel or auto strike or with the labor racketeering scandals in the last years of the decade. On the other hand, race relations, of extreme importance in the 1960s, made, except for the Little Rock crisis of 1957 and an occasional election-time outburst, little claim to public attention before that time. From the late 1940s into the mid-1950s sundry spy and Communist hunts were of concern, but the issue fairly well fizzled after that. Other issues which might be mentioned had even briefer or more erratic days in the sun: the food shortage of 1947, the MacArthur hearing of 1951, various space flights. Similarly, personal crisis for the Presidents such as heart attacks and major surgery for Presidents Eisenhower and Johnson and the attempted assassination of President Truman could not readily be fashioned into a predictor variable. In any event, these events seem to have far more impact on the stock market than on popularity ratings.

Scandal is a recurring feature of public awareness and thus is more promising as a potential variable in the analysis. Besides the scandals associated with alleged spies and Communists in the government during the McCarthy era and those associated with labor in the late 1950s, Americans, with greatly varying degrees of pain, have suffered through the five percenter scandal of 1949–50; charges of corruption in the RFC in 1951, in the Justice Department in 1952, and in the FHA in 1954; and scandals over Sherman Adams in 1958, over television quiz shows in 1959, over industry "payola" in the late 1950s, over Billie Sol Estes in 1962, and over Bobby Baker in 1963. While scandal is never worked into the regression analysis some preliminary suggestions as to its relevance to a "moral crisis" phenomenon which may in turn affect Presidential popularity are developed in Section VI below.

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[30] A. T. Steele, *The American People and China* (New York: McGraw-Hill, 1966), p. 294. In May 1964 Gallup found almost two-thirds of the population said they paid little or no attention to developments in South Vietnam. Lloyd A. Free and Hadley Cantril, *The Political Beliefs of Americans* (New York: Clarion, 1968), pp. 59–60.

[31] Although the Korean War continued into President Eisenhower's administration, he is not "blamed" for the war in the analysis since of course he was elected partly because of discontent over the war. Accordingly the war variable is set at zero for this period.

Some thought was given to including a "lame duck" variable when it was observed that the popularity of Presidents Truman and Johnson rose noticeably after they decided not to seek third terms. The trouble is, however, that President Eisenhower was a lame duck for his entire second term and it was found easier to ignore the whole idea than to decide what to do about this uncomfortable fact.

One domestic variable which did show some very minor promise was a dummy variable for the presence of a major strike. The variable takes on a zero value almost everywhere except in parts of President Truman's first term. After that time major strikes were rather unusual and, when they did occur, usually lasted for such a short time that there was barely time to have a public opinion survey conducted to test their effects. Despite these peculiarities, the variable did show statistical significance, though only after the Korean War dummy had been incorporated in the equation to allow for a major peculiarity of President Truman's *second* term. Substantively the variable suggests a popularity drop of less than three percentage points when a major strike is on and, as such a minor contributor, it is not included in the discussion below. Its small success, however, may suggest that further experimentation with the effects of specific domestic events could prove profitable.

## III. RESULTS WITHOUT THE WAR VARIABLE

*In summary the expected behavior of Presidential popularity is as follows. It is anticipated 1) that each President will experience in each term a general decline of popularity; 2) that this decline will be interrupted from time to time with temporary upsurges associated with international crises and similar events; 3) that the decline will be accelerated in direct relation to increases in unemployment rates over those prevailing when the President began his term, but that* improvement *in unemployment rates will not affect his popularity one way or the other; and 4) that the President will experience an additional loss of popularity if a war is on.*

In this section the relation of the first three variables to Presidential popularity will be assessed. In the next section the war variable will be added to the analysis.

The association between the first three variables and Presidential popularity is given in its baldest form in equation 1 in Table 1.[32] The equation explains a respectable, if not sensational, 22 percent of the variance. The coalition of minorities variable shows, in conformity with the speculation above, a significant negative relationship. The equation suggests that, in general, a President's popularity rating starts at 69 percent and declines at a rate of about six percentage points per year.

However, while the coefficients for the rally round the flag and economic slump variables are in the expected direction, they are not significant either in a statistical or a substantive sense. The trouble with the economic slump variable was anticipated in the discussion about it in Section II: the economic decline occurred during the relatively popular reign of President Eisenhower; while the slump seems to have hurt his popularity, even with the decline he remained popular compared to other Presidents; hence what is needed is a variable to take into account this peculiar "Eisenhower effect."

To account for this phenomenon, equation 2 mixes into the analysis a dummy variable for each of the Presidents. This formulation insists that all Presidents must decline (or increase) in popularity at the same rate but, unlike equation 1, it allows each President to begin at his own particular level. Thus peculiar effects of personality, style, and party and of differences in the conditions under which the President came into office can be taken into account.[33]

The addition improves things considerably.

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[32] Each equation is displayed vertically. The dependent variable, the percentage approving the way the President is handling his job, has a mean of 57.5 and a standard deviation of 14.8. The number of cases is 292. The figures in parentheses are the standard errors for the respective partial regression coefficients. To be regarded statistically significant a regression coefficient should be, conventionally, at least twice its standard error. All equations reported in this study are significant (F test) at well beyond the .01 level. The Durbin-Watson *d* is an indicator of serial correlation which suggests decreasing positive serial correlation as the statistic approaches the value of 2.0. All equations in this study exhibit a statistically significant amount of positive serial correlation.

[33] The dummy variables formalize the sort of discussion found in Neustadt, *op. cit.*, p. 98. They account for what a singer might call tessitura.

TABLE 1. REGRESSION RESULTS INCLUDING ADMINISTRATION EFFECTS

| | Equations | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Intercept | 69.37 | 54.51 | 68.15 | 71.52 |
| Independent variables | | | | |
| Coalitions of minorities (in years) | −6.14 | −5.12 | | |
| | (0.71) | (0.48) | | |
| Rally round the flag (in years) | −0.31 | −2.15 | −1.87 | −2.63 |
| | (1.95) | (1.35) | (1.07) | (1.07) |
| Economic slump (in % unemployed) | −0.09 | −3.18 | −5.30 | −5.86 |
| | (0.96) | (0.75) | (0.60) | (0.60) |
| Dummy variables for administrations | | | | |
| Eisenhower | | 24.08 | 0.51 | |
| | | (1.42) | (2.29) | |
| Kennedy | | 23.87 | 11.33 | |
| | | (1.82) | (2.96) | |
| Johnson | | 10.46 | 4.26 | |
| | | (1.54) | (2.48) | |
| Coalition of minorities variable for administrations (in years) | | | | |
| Truman | | | −11.44 | −12.54 |
| | | | (0.84) | (0.50) |
| Eisenhower | | | 0.83 | 0.13 |
| | | | (0.57) | (0.47) |
| Kennedy | | | −5.96 | −1.58 |
| | | | (1.36) | (0.82) |
| Johnson | | | −9.12 | −8.68 |
| | | | (0.67) | (0.50) |
| $d$ | .13 | .30 | .46 | .44 |
| Standard error of estimate | 13.16 | 8.88 | 6.86 | 7.06 |
| $R^2$ | .22 | .65 | .79 | .78 |

The fit is much better and the rally round the flag and economic slump variables attain respectable magnitudes in the predicted direction, although the rally round the flag variable does not quite reach statistical significance.

The equation suggests that the Presidents have declined at an overall rate of over five percentage points per year but that each has done so at his own particular level. President Truman's decline is measured from a starting point of 54.51 percent (when the dummy variables for the Eisenhower, Kennedy, and Johnson administrations are all zero). President Eisenhower declines from a much higher level, about 79 percent (54.51 + 24.08), President Kennedy from 78 percent, and President Johnson from 65 percent.

The importance of these dummy variables clearly demonstrates that *an analysis of Presidential popularity cannot rely entirely on the variables discussed in Section II, but must also incorporate parameters designed to allow for the special character of each administration.* To an extent this is unfortunate. The beauty of equation 1 is that it affords a prediction of a President's popular rating simply by measuring how long he has been in office, how long it has been since the last rally point, and how many people are unemployed. Such predictions, however, would be quite inaccurate because the fit of the equation is rather poor. Instead one must include the administration variables, the magnitudes of which cannot be known until the President's term is over. So much for beauty.

In equation 3 administration effects are incorporated in a different manner, greatly improving fit and reducing serial correlation. In this formulation each President is allowed to begin at his own level of popularity as in equation 2, but in addition each may decline (or increase) at his own rate: for each administration there is a different coefficient for the coalition of minorities variable. Three of the four values so generated are strongly significant while the magnitudes of the administration dummies drop greatly. When the administration dummies are dropped entirely from consideration, as in equation 4, the regression coefficients mostly remain firm and the fit of the equation is scarcely weakened. It is clear that *the important differences between administrations do not lie so much in different overall levels of popularity, but rather in the widely differing rates at which the coalition of minorities variable takes effect.*

The popular decline of Presidents Truman and Johnson has been almost precipitous. President Truman's rating fell off at some 11 or 12 percentage points per year while President Johnson declined at a rate of around 9 points a year. President Kennedy was noticeably more successful at holding on to his supporters. Then there is the Eisenhower phenomenon: in spite of all the rationalizations for the coalitions of minorities concept tediously arrayed in Section II, President Eisenhower's rating uncooperatively refuses to decline at all.[34]

In equation 3, Presidents who served two terms were required to begin each term at the same level and their rate of decline or increase also had to be the same in each term. Liberation from these restrictions is gained in the rather cluttered equation 5 of Table 2 which is like equation 3 except that it affords a term by term, rather than simply an administration by administration comparison. As can be seen President Eisenhower managed a statistically significant *increase* of popularity of some two and a half percentage points per year in his first term. His second term ratings showed a more human, but very minor and statistically non-significant decline.[35]

No important differences emerge in the Eisenhower phenomenon when the economic slump variable, which functions mainly during the Eisenhower years, is dropped from the equation.

No matter how the data are looked at then the conclusion remains the same. *President Eisenhower's ability to maintain his popularity, especially during his first term, is striking and unparallelled among the postwar Presidents.* An examination of some of the possible reasons for this phenomenon is conducted in Section VI below.

The rally round the flag and the economic slump variables emerge alive and well in equations 3, 4, and 5 (and 6). Both are usually statistically significant but their substantive importance varies as one moves from an administration by administration formulation of the coalition of minorities variable (equations 3 and 4) to the term by term formulation in Table 2. Specifically, the rally round the flag variable gets stronger while the economic slump variable weakens.

The rally round the flag variable is very much a parasite—it is designed to explain bumps and wiggles on a pattern measured mainly by the other variables. Consequently the rally round the flag variable does very poorly on its own and only begins to shine when the overall trends become well determined by the rest of

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[34] It was noted in Section I that some minor bias in these results is introduced by an embellished rate of "no opinion" in the first weeks of the Kennedy and first Eisenhower terms. As this rate declined, there was some tendency for the Presidents' approval *and* disapproval rates to rise. To see if this peculiarity had any major impact, equations 3 and 4 were recalculated using the percentage *disapproving* as the dependent variable. This manipulation causes no fundamental differences, although President Eisenhower's rating behaves a little less outrageously.

[35] If the term dummies are dropped from the equation to attain a version comparable to equation 4, the Eisenhower phenomenon holds except that his first term increase drops to 2.00 (still significant) and his second term decrease is a slightly steeper —0.36 (still not significant).

TABLE 2. REGRESSION RESULTS INCLUDING TERM EFFECTS AND THE WAR VARIABLES

| | Equations | |
|---|---|---|
| | (5) | (6) |
| Intercept | 72.00 | 72.38 |
| Independent variables | | |
| Rally round the flag (in years) | −4.88 | −6.15 |
| | (1.04) | (1.05) |
| Economic slump (in % unemployed) | −2.67 | −3.72 |
| | (0.65) | (0.65) |
| Dummy variables for terms | | |
| Truman—second | −15.25 | −12.41 |
| | (3.69) | (3.57) |
| Eisenhower—first | −3.17 | −2.41 |
| | (3.15) | (3.02) |
| Eisenhower—second | −5.30 | −4.35 |
| | (3.07) | (2.94) |
| Kennedy | 7.53 | 7.18 |
| | (3.29) | (3.14) |
| Johnson—first | 7.14 | 6.77 |
| | (5.50) | (5.26) |
| Johnson—second | −1.15 | −0.79 |
| | (3.06) | (3.25) |
| Coalition of minorities variable for terms (in years) | | |
| Truman—first | −9.21 | −8.93 |
| | (1.41) | (1.35) |
| Truman—second | −7.98 | −2.83 |
| | (1.00) | (1.37) |
| Eisenhower—first | 2.45 | 2.58 |
| | (0.85) | (0.82) |
| Eisenhower—second | −0.07 | 0.22 |
| | (0.65) | (0.62) |
| Kennedy | −5.11 | −4.76 |
| | (1.21) | (1.16) |
| Johnson—first | −4.98 | −3.71 |
| | (14.01) | (13.39) |
| Johnson—second | −8.15 | −8.13 |
| | (0.66) | (0.80) |
| Dummy variables for wars | | |
| Korea | | −18.19 |
| | | (3.43) |
| Vietnam | | −0.28 |
| | | (2.79) |
| $d$ | .57 | .67 |
| Standard error of estimate | 6.07 | 5.80 |
| $R^2$ | .84 | .86 |

the equation. In the end, *the rally round the flag variable suggests a popularity decline of around five or six percentage points for every year since the last rally point*—about the same magnitude as the coalition of minorities variable in its general state as in equations 1 and 2.

The declining fortunes of the economic slump variable suggest that the variable in equations 3 and 4 was partly covering for the differences between the two Eisenhower terms: the first term was associated with increasing popularity and a smaller recession, the second with somewhat declining popularity and a larger recession. With the Eisenhower terms more thoroughly differentiated in equation 5, the variable is reduced to a more purely economic function. The magnitude of the coefficient of the *economic slump variable* in this equation *suggests a decline of popularity of about three percentage points for every percentage point rise in the unemployment rate over the level holding when the President began his present term.* Since the unemployment rate has varied in the postwar period only from about 3 to 7 percent, the substantive impact of the economic slump variable on Presidential popularity is somewhat limited.

## IV. RESULTS WITH THE WAR VARIABLE ADDED

The variable designed to tap the impact on Presidential popularity of the wars in Korea and Vietnam was applied with no great confidence that it would prove to have an independent, added effect when the coalition of minorities had already been incorporated into the equation especially given the problem of multicollinearity. It is obvious from a perusal of a plot that, as noted in Section II and as demonstrated in equation 5, Presidents Truman and Johnson were in popular decline during their warless first terms.[36] Furthermore each was in clear decline in the first part of his second term before the wars started and it is not at all obvious that this trend altered when the wars began.

The equations suggest otherwise, however. When a war dummy was appended to the equations already discussed, it emerged significant and suggested that the presence of war depressed the popularity of Presidents Truman and Johnson by over seven percentage points.

The next step, obviously, was to set up a separate dummy variable for each war. This brought forth the incredible result documented in equation 6: *the Korean War had a large, significant, independent negative impact on President Truman's popularity of some 18 percentage points, but the Vietnam War had no independent impact on President Johnson's popularity at all.*

Confronted with a result like this, one's first impulse is to do something to make it go away. This impulse was fully indulged. Variables were transformed and transmuted, sections of the analysis were reformed or removed, potentially biassing data were sectioned out. But nothing seemed to work. The relationship persisted. In fact under some manipulations the relationship became stronger.

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[36] Regression statistics relating to President Johnson's first term are very unreliable, as the size of the standard errors suggests, because the popularity question was posed so few times during this brief period.

One's second impulse, then, is to attempt to explain the result. One speculates.

The wars in Korea and Vietnam differed from each other in many respects, of course, but it seems unlikely that these differences can be used in any simple manner to explain the curious regression finding. This is the case because, as one study has indicated, public response to the wars themselves was much the same. Support for each war, high at first, declined as a logarithmic function of American casualties—quickly at first, then more slowly. The functions for each of the wars for comparable periods were quite similar. Furthermore both wars inspired support and opposition from much the same segments of the population.[37]

Therefore it is probably a sounder approach in seeking to explain the regression finding to look specifically at popular attitudes toward *the President's relation to the war,* rather than to perceptions of the war itself. A comment by Richard Neustadt seems strikingly relevant in this respect. "Truman," he observes, "seems to have run afoul of the twin notions that a wartime Chief Executive ought to be 'above politics' and that he ought to help the generals 'win.' "[38]

President Johnson seems to have run considerably less afoul. In seeking to keep the war "above politics," he assiduously cultivated bipartisan support for the war and repeatedly sought to demonstrate that the war effort was simply an extension of the policies and actions of previous Presidents. He was especially successful at generating public expressions of approval from the most popular Republican of them all: General Eisenhower. Vocal opposition to the war in Vietnam came either from groups largely unassociated with either party or from members of the President's own party. Then, when the latter opposition began to move from expressions of misgivings at congressional hearings to explicit challenges in the primaries, President Johnson removed himself from the battle precisely, he said, to keep the war "above politics." And, while there were occasional complaints from the right during Vietnam that President Johnson had adopted a "no win" policy there, these were continually being undercut by public statements from General William Westmoreland—a man highly respected by the right—insisting that he was receiving all the support he needed from the President and was getting it as fast as he needed it.

If these observations are sound, the single event which best differentiates the impact of the Korean and Vietnam wars on Presidential popularity was President Truman's dismissal of General Douglas MacArthur. That move was a major factor in the politicization of the war as Republicans took the General's side and echoed his complaints that it was the President's meddling in policy that was keeping the war from being won.[39]

The differing impact of the wars on Presidential popularity therefore may be due to the fact that Korea became "Truman's war" while Vietnam never in the same sense really became "Johnson's war."[40]

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[37] John E. Mueller, "Patterns of Popular Support for the Wars in Korea and Vietnam," unpublished paper, Department of Political Science, University of Rochester, 1969.

[38] *Op. cit.,* p. 97.

[39] See John W. Spanier, *The Truman-MacArthur Controversy and the Korean War* (Cambridge, Mass.: Belknap, 1959); also Neustadt, *op. cit.,* passim; and Trumbull Higgins, *Korea and the Fall of MacArthur* (New York: Oxford, 1960). See also the data in George Belknap and Angus Campbell, "Political Party Identification and Attitudes Toward Foreign Policy," 15 *Public Opinion Quarterly* 601–23 (Winter 1951–52). Note especially the strong party polarization on the issue. That the public was strongly inclined to support General MacArthur in the dispute can be seen from poll data. The first polls, conducted as the General was making his triumphal, "old soldiers never die" return to the United States in April 1951, suggest more than twice as many people supported the General as supported the President. As Neustadt suggests (*op. cit.,* p. 97), emotion on the issue faded during the Senate Hearings on the issue which lasted until June and this seems to have been to the benefit of President Truman's position. The Truman point of view received its greatest support in late June and early July as peace talks were being begun. As the talks began to prove unproductive, however, public opinion began to revert to its previous support of General MacArthur, until, by the first days of 1952 (when the polling agencies grew bored with the issue), the MacArthur position was as strongly approved and President Truman's as strongly rejected as ever.

[40] There is evidence which suggests that World War II, a much more popular (and much larger) war than either Korea or Vietnam, may have worked to the distinct *benefit* of President Roose-

One other item of speculation might be put forth. Domestically, the war in Vietnam was accompanied by a profoundly important crisis as America confronted its long-ignored racial dilemma head on. There seems to have been nothing comparable during the Korean War. The clamor associated with McCarthyism comes to mind but many analysts feel that, however important to politicians, intellectuals, and journalists, McCarthyism was of rather less than major concern to public opinion.[41] Furthermore its dramatic climax, the Army-McCarthy hearings, took place months after the Korean War had ended and over a year after President Truman left office.

It may be, then, that the discontent associated with the racial crisis was enough by itself to cause much of President Johnson's popular decline and thus that the unhappiness over the Vietnam War could make little additional inroad. In the Truman case, there was no profound independent domestic source of discontent: his second term coalition of minorities decline is usually found as in equation 5 to have been less than his first term decline and when, as in equation 6, a variable has already accounted for the war effect, his decline is quite moderate. Thus in a sense there was "room" for the war to have an independent impact.

It would be wise in concluding this section to emphasize what has and what has not been said. It has *not* been argued that the war in Vietnam had nothing to do with President Johnson's decline in popularity and thus the analysis cannot really be used to refute the President's own estimation of the impact of Vietnam as indicated in the quotation that heads this study. However it is argued that whatever impact the war had was tapped by the other variables in the equation, especially the coalition of minorities variable which is specifically designed to account for general overall decline. When the same sort of analysis is applied in the Korean period it is found that a variable associated with the Korean War does show significance even after other variables have been taken into account. What the regression analysis shows therefore is that, while the Korean War does seem to have had an *independent, additional* impact on President Truman's decline in popularity, the Vietnam War shows no such relation to President Johnson's decline.

## V. THE RESIDUALS

An analysis of the residuals finds that equation 6 predicts worst in President Truman's first term. The President's extremely high initial ratings are not well predicted suggesting that the equation does not adequately account for the trauma of President Roosevelt's death[42] combined as it was with the ending of World War II and with important peace conferences. It was almost as if Americans were afraid to disapprove of President Truman.

From these spectacular highs, President Truman plunged to great lows during the labor turmoil of 1946. These ratings are also badly specified by the equation. The Truman popularity rose in early 1947, as the labor situation eased, and then declined for the rest of the term. Thus while President Truman's first term, like the other Democratic terms, shows an overall decline of popularity, that decline was considera-

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velt. The National Opinion Research Center in its post-election poll in 1944 asked Roosevelt supporters if they would have voted for Dewey "if the war had been over." Enough answered in the affirmative to suggest that Dewey might well have won in a warless atmosphere. From data supplied by the Inter-University Consortium for Political Research.

[41] Samuel A. Stouffer, *Communism, Conformity, and Civil Liberties* (Garden City, N.Y.: Doubleday, 1955), especially ch. 3; Campbell, *et al., op. cit.*, pp. 50–51; Nelson W. Polsby, "Toward an Explanation of McCarthyism," 8 *Political Studies* 250–71 (October 1960); Elmo Roper, *You and Your Leaders* (New York: Morrow, 1957), pp. 250–51.

[42] In late November 1945, over six months after President Roosevelt's death, Gallup asked his sample, "In your opinion, who is the greatest person living or dead, in world history?" Fully 28 percent proffered Roosevelt's name. Abraham Lincoln was mentioned by 19 percent, Jesus Christ by 15 percent, and George Washington by 8 percent. No one else received more than 2 percent. And the aura lasted. A survey conducted in June 1949 in the city of Philadelphia (which had voted 59 percent for Roosevelt in 1944 as against a national rate of 55 percent) posed this question: "Could you tell us the name of a great person, living or dead, whom you admire the most?" The most commonly mentioned names were Roosevelt with 42 percent, Lincoln with 9 percent, and Washington with 5 percent. (The absence of Jesus Christ on this latter list presumably can be laid to the peculiarities of question wording—or of Philadelphians.) Fillmore H. Sanford, "Public Orientation to Roosevelt," 15 *Public Opinion Quarterly* 190–91, 200 (Summer 1951). In 1948, Roper found 43 percent of a national sample offering Roosevelt's name when queried, "Considering all the men in America who have been prominent in public affairs during the past 50 years, which one or two have you admired the most?" Dwight Eisenhower was second at 17 percent. E. Roper, *op. cit.*, p. 22.

bly more erratic than the others. The dummy variable for strikes, discussed briefly in Section II, improved matters only slightly.

Beyond this, the residuals are reasonably well behaved. There are small but noticeable effects from the lame duck phenomenon at the end of the Truman and Johnson administrations and from the "no opinion" peculiarity of the initial weeks of the Eisenhower and Kennedy administrations. And here and there are data points whose magnitudes have somehow managed to escape specification by the variables in the regression equation. One can of course generate a unique explanation for each of these but this procedure clutters the analysis more than it is worth. Besides, the laws of sampling insist that Gallup must have made *some* mistakes.

As the magnitude of the Durbin-Watson $d$ indicates, serial correlation has by no means been eliminated in the regression equations. Allowing the coalition of minorities variable to be specified for each term improved things considerably, but much is left to be desired.

### VI. THE EISENHOWER PHENOMENON

Great noise was made in Section II about the coalition of minorities variable with its stern prediction that a President's popularity would decline inexorably over his four year term. The noise was not entirely unjustified since the variable proved to be a hardy and tenacious predictor for the postwar Democratic administrations.

The variable fails for the Eisenhower administration, however, especially for the General's first term. The analysis suggests then that if a President wants to leave office a popular man he should either 1) be Dwight David Eisenhower, or 2) resign the day after inauguration.

The Eisenhower phenomenon, noted but left dangling without explanation or rationalization in Section III, deserves special examination. Why didn't President Eisenhower decline in popularity like everybody else? A number of suggestions can be proffered.

1. To begin with, credit must be given to President Eisenhower's *personal appeal:* he was extremely likeable—a quality very beneficial in a popularity contest and one lacked in abundance by, say, Lyndon Johnson. As Fillmore Sanford has observed, "The American people, in reacting to a national leader, put great emphasis on his personal warmth"[43]—a quality projected to an unusual degree by President Eisenhower. As part of this, he was able to project an image of integrity and sincerity which many found to be enormously attractive.[44]

2. Early in his first term President Eisenhower was able to present to the public one sensational achievement: he *ended the Korean War* —or, at any rate, presided over its end. This accomplishment was seen by the public as he left office to be a great one[45] and was used with profit by the Republicans in a Presidential campaign a full 15 years after it happened. From the standpoint of public opinion it may well have been the most favorable achievement turned in by any postwar President. As such it may have tended to overwhelm the negative impact of anything else the President did, at least for the first years of his administration. Some credit for this is given in the regression analysis since the signing of the truce is counted as a rally point, but this may be a totally inadequate recognition.

There is another aspect of President Eisenhower's first term which may not be sufficiently accounted for in the rally round the flag variable: the euphoria of the "spirit of Geneva" period toward the end of the term when the President's popularity should have been at its lowest ebb.

3. President Eisenhower's *amateur status* may also have worked to his benefit, at least for a while. The public may have been more willing to grant him the benefit of a doubt, to extend the "honeymoon" period, than it would for a President who is a political professional. It is also easier under these circumstances for the President to appear "above the battle" and thus to be blamed only belatedly and indirectly for political mishaps, thereby softening their impact.

4. President Eisenhower may have been curiously benefited by the fact that, especially on the domestic front, *he didn't do anything.*[46] In-

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[43] *Op. cit.*, p. 198.

[44] See Philip E. Converse and Georges Dupeux, "De Gaulle and Eisenhower: The Public Image of the Victorious General," in Angus Campbell, *et al.*, *Elections and the Political Order* (New York: Wiley, 1966), pp. 292–345.

[45] In December 1960 the public was asked what it felt was Eisenhower's greatest accomplishment. The ending of the Korean War was mentioned by 11 percent and a related comment, "he kept us out of war," was suggested by an additional 32 percent. No other specific accomplishment was mentioned by more than 5 percent; only 3 percent mentioned anything having to do with the domestic scene. See also Neustadt, *op. cit.*, p. 98.

[46] As Irving Kristol argues, ". . . when a conservative administration does take office, it pursues no coherent program but merely takes satisfaction in not doing the things that the liberals may be clamoring for. This, in effect, is what happened during the two terms of President Eisen-

deed analysts of the Eisenhower administration often argue that its contribution lies in what it *didn't* do. The times called for consolidation, they argue, and President Eisenhower's achievement was that he neither innovated nor repealed, but was content to preside over a period of placidity in which he tacitly gave Republican respectability to major Democratic innovations of earlier years: the programs of the New Deal domestically and the policies of the Truman Doctrine internationally.[47]

In terms of the justification for the coalition of minorities variable as discussed in Section II, such behavior could have a peculiar result. It was assumed in part that the President would enact programs which, while approved by the majority, would alienate intense minorities which would gradually cumulate to his disadvantage. But suppose the President doesn't do anything. Those who want no change are happy while, if things are sufficiently ambiguous, those who support change have not really been denied by an explicit decision and can still patiently wait and hope. At some point of course those who want change begin to see that they are never going to get their desires and may become alienated, but this will be a delayed process. At least in moderate, placid times, a conservative policy may dissipate some of the power of the coalition of minorities phenomenon. Were polls available, one might find that President Warren Harding maintained his popularity as strikingly as President Eisenhower.

5. Although it might be difficult to sort out cause and effect, it is worth noting that President Eisenhower's first term (and most of his second) coincided with a *period of national goodness*. In a brilliant article Meg Greenfield has noted that "moral crises" as appraised and bemoaned by intellectuals seem to follow a cyclic pattern: we go through a period in which the popular journals are filled with articles telling us how bad we are after which there is a period of respite.[48]

Miss Greenfield's main indicator of these ethical cycles is exquisite: the number of items under the heading, "US: Moral Conditions," in the *Readers' Guide to Periodical Literature*. The pattern, elaborated and duly pedantified, is given in Table 3. As she notes, our first moral crisis in the postwar period arose in the early 1950s and was associated with " 'five percenters,' deep freezes, mink coats, the Kefauver hearings, and a series of basketball fixes," while "the symbols of our present [1961] decline are Charles Van Doren, payola, cheating in school, and the decision of Frances Gary Powers not to kill himself." We never recovered as thoroughly from that crisis as we did from the earlier one for, as the crisis showed signs of waning (the success of the Peace Corps began to show how good we were at heart), new elements—Billie Sol Estes, Bobby Baker, President Kennedy's assassination, campus revolts, the hippies, and the city riots—proved once again that we have a "sick society." Our moral crises are regenerated every eight years and seem to coincide with the end of Presidential administrations.[49]

TABLE 3. THE GREENFIELD INDEX

| Number of Items under the Heading, "US: Moral Conditions" in *Readers' Guide to Periodical Literature,* 1945–1968, by year | |
|---|---|
| 1945 | 1 |
| 1946 | 0 |
| 1947 | 8 |
| 1948 | 1 |
| 1949 | 1 |
| 1950 | 3 |
| 1951 | 35 |
| 1952 | 17 |
| 1953 | 1 |
| 1954 | 4 |
| 1955 | 2 |
| 1956 | 0 |
| 1957 | 7 |
| 1958 | 0 |
| 1959 | 9 |
| 1960 | 32 |
| 1961 | 23 |
| 1962 | 10 |
| 1963 | 11 |
| 1964 | 5 |
| 1965 | 6 |
| 1966 | 7 |
| 1967 | 18 |
| 1968 | 10 |

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hower . . ." "The Old Politics, the New Politics, and the *New,* New Politics," *New York Times Magazine,* November 24, 1968, p. 167.

[47] See Clinton Rossiter, *The American Presidency* (New York: Harcourt, Brace, 1960), pp. 161–78.

[48] Meg Greenfield, "The Great American Morality Play," *The Reporter,* June 8, 1961, pp. 13–18.

[49] It may, or then again may not, be worth noting that Presidential elections in which the incumbent party was removed, 1952, 1960, and 1968, occurred during moral crises while the elections in which the President was retained, 1948, 1956, and 1964, all took place during times of relative goodness.

Of course objective indicators of public morality have not been careening in this manner. Much of the fluctuation in the Greenfield index is no doubt due to journalistic fad. A sensational fraud, scandal or disruption causes theologians, journalists, and other intellectuals to sociologize: society is sick. Others pick up the idea and it blossoms into a full moral crisis. In a year or two the theme no longer sells magazines and the space is filled with other profundities. Fraud, scandal, and disruption continue, but the moral crisis eases.

But—and this is a logical and empirical leap of some magnitude—to the extent that these patterns reflect and influence public attitudes, they may be relevant to Presidential popularity. The early Eisenhower years are notable for their absence of moral anguish and they differ from other between-crisis periods in an important respect: not only were we not demonstrably bad, we were positively good for we were undergoing a religious revival. Miss Greenfield looked at the items under the heading, "US: Religious Institutions." She finds only six items in the 1951–53 period, but 25 in 1953–55 while "in the 1955–57 volume, at the height of our virtue . . . the religious listings reached thirty-four with twenty-eight 'see alsos.' "

If we were so good ourselves, how could we possibly find fault in our leader?

## VII. FURTHER RESEARCH

This study has been reasonably successful at generating a regression equation based on only four rather simple variables, which fits quite well the erratic behavior over 24 years of the Presidential popularity index. There is, however, much room for improvement and refinement.

Little has been done to separate out from the coalition of minorities variable the specific and divergent influences of domestic events on Presidential popularity. There was one variable designed to account in a general way for changes in the economy, some limited analysis was made of the relevance of major strikes, and comments were interjected about the role of scandal and "moral crisis." But domestic life is considerably more complicated than this and more precise social, political, and economic indicators can be sought.

It would also be of value to get better estimates of the impact of different *kinds* of international events on Presidential popularity—although, as already suggested, such analysis may find that all dramatic international events affect popularity in much the same way no matter how they may differ in historical significance.[50]

The analysis strongly suggests that Presidential style as well as the ideological and political nature of the administration and the times can make a sizeable difference in the way popularity ratings behave. A more precise assessment of these relationships would be most desirable.

The study has dealt entirely with general popular approval of the President. Left unexamined are the ways population groups differ in their approach to the President. Supporters of the President's own party, for example, are more likely to approve the way he handles his job. Presumably they are also relatively hard to alienate, are more likely to be enchanted by his successes, and are more tolerant of his blunders.

It should also be possible to extend the analysis to other bodies of data. Somewhat comparable data from the Roosevelt administration are available. Although the popularity question was posed with far less regularity in those days (and was largely dropped during Warld War II) and although there are problems with varying question wording, students of President Roosevelt's popularity ratings emerge with findings which fit well with those of this study.[51] The popularity ratings of Governors and Senators in states with active statewide polls can also be analyzed as can data on national leaders from such countries as Britain, Canada, and France.[52]

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[50] It may prove valuable to attempt to see how spectacular and cumulative international events and shifts in governmental policy—to use the distinction made by Karl Deutsch and Richard Merritt—differ in impact. "Effects of Events on National and International Images" in Herbert C. Kelman (ed.) *International Behavior* (New York: Holt, 1965), pp. 132–87.

[51] Wesley C. Clark has found some relation between the Roosevelt popularity and the state of the economy in the 1937–1940 period. He also notes a general "downward slant" in the rating over time and finds a rise of popularity during international crises. ("Economic Aspects of a President's Popularity," Ph.D. Dissertation, University of Pennsylvania, 1943, pp. 41, 28, 35). See also B. Roper, *op. cit.*, and E. Roper, *op. cit.*, chapters 2 and 3. And V. O. Key has observed that during 1940 "the popularity of Roosevelt rose and fell with European crises." *Politics, Parties and Pressure Groups* (New York: Crowell, 1952, 3rd. ed.), p. 596 (cited in Waltz, *op. cit.*, p. 272.)

[52] British observers have noted an apparent relation between unemployment and party preference in their country: rising unemployment seems to have benefited Labor while declining unemployment favors the Tories. Henry Durant, "Indirect Influences on Voting Behavior," 1 *Polls* 7–11 (Spring 1965). Extensive data from France on the popularity of President De Gaulle have been

## VIII. SUMMARY

This investigation has applied multiple regression analysis to the behavior of the responses to the Gallup Poll's Presidential popularity question in the 24 year period from the beginning of the Truman administration to the end of the Johnson administration. Predictor variables include a measure of the length of time the incumbent has been in office as well as variables which attempt to assess the influence on his rating of major international events, economic slump, and war. Depite the austerity of this representation of a presumably complex process, the fit of the resulting equation was very good: it explained 86 percent of the variance in Presidential popularity.

This degree of fit could only be attained, however, by allowing the special character of each Presidential administration to be expressed in the equation. Thus it does not seem possible to predict a given President's popularity well simply by taking into account such general phenomena as the state of the economy or of international affairs.

The first variable, dubbed the "coalition of minorities" variable, found, as expected, the popularity of most Presidents to be in decline during each term. The important differences between administrations do not lie so much in different overall levels of popularity, but rather in the widely differing rates at which this coalition of minorities variable takes effect. Specifically, the popular decline of Presidents Truman and Johnson was quite steep while President Kennedy seems to have been somewhat better at maintaining his popularity. President Eisenhower's popularity did not significantly decline at all during his second term and actually increased during his first term.

In considering this Eisenhower phenomenon it is suggested that a combination of several causes may be relevant: the President's personal appeal, his ending of the Korean War, his amateur status, his domestic conservatism at a time when such a policy was acceptable, and his fortune in coming to office at a time of national goodness.

The second variable, the "rally round the flag" variable, predicts short term boosts in a President's popularity whenever there occurs an international crisis or a similar event. The variable proves to be a sturdy one and suggests a popular decline of about five or six percentage points for every year since the last "rally point."

Economic effects were estimated in the third variable. The variable could only be made to function if it was assumed that an economy in slump harms a President's popularity, but an economy in boom does not help his rating. A decline of popularity of about three percentage points is suggested for every percentage point rise in the unemployment rate over the level holding when the President began his present term.

The fourth variable attempted to take into account the influence of war on Presidential popularity. It was found that the Korean War had a large, significant independent negative impact on President Truman's popularity of some 18 percentage points, but that the Vietnam War had no independent impact on President Johnson's popularity at all. It is suggested that this difference may be due to the relationship between the Presidents and the wars: President Truman was less able than President Johnson to keep the war "above" partisan politics and he seemed to the public to be interfering and restraining the generals. The absence in the Truman case of a domestic crisis comparable to the racial turmoil of the Johnson era may also be relevant.

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published: for example, *Gallup Opinion Index*, March 1968, pp. 27–28. A study by Howard Rosenthal has investigated regional aspects of the General's popularity. "The Popularity of Charles De Gaulle: Findings from Archive-based Research," 31 *Public Opinion Quarterly* 381–98 (Fall 1967).

# ON CIVIL DISOBEDIENCE IN RECENT AMERICAN DEMOCRATIC THOUGHT*

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Theoretical discussions of civil disobedience on ethical and political grounds received special attention in this country during the Nuremberg trials, the security and loyalty controversies of the 1950's and the pre-arms control years of nuclear power. A fourth wave of interest formed after the early civil rights protests and a fifth is appearing to consider dissent from national policies on the Vietnam War. In this paper civil disobedience is viewed from a trough between the fourth and most recent wave. The phenomenon is interpreted with selected ideas from the study of political obligation[1] and unconventional dissent. The essay first assesses recent American analysis of civil disobedience to determine what the criteria should be to distinguish it from other forms of political action and to discover its political ethics. Secondly, there is an attempt to answer the question: Is there any appreciable service that carefully defined civil disobedience might perform in American democratic thought? The complete enterprise is provoked by a need to examine new strategies for democratic citizenship in a time when the deficiencies of American political life are becoming known to increasing numbers and varieties of people instead of remaining the preserve of enlightened elites.

## I. CURRENTS OF THOUGHT

The rule-breaking aspect of "civil disobedience," a term usually credited to Thoreau, offers fewer difficulties for an examination of the topic than the "civil" feature. Typically, "disobedience" is breaking a legal norm that has authoritative sanction. The norm does not have to be a state law but might be a rule of a subsidiary group, such as the university. which in American constitutional theory and practice has the right to make and enforce internal regulations, subject to the state's writ and charter. Although this discussion has implications for disobedience within subsidiary groups, in the main it revolves around citizen resistance to the laws and policies of the state. "Civil" is open to several interpretations. These five meanings cover most possibilities:[2] 1) recognition of citizen obligations for the existing legal order; 2) the opposite of military; 3) civilized or moral; 4) public instead of private; and 5) affecting the political society. All of these, but especially 1), 3) and 5), are directly pertinent for the objectives of this paper. The meanings of both parts of the term become clarified when criteria are examined.

Many of the suggestions for the criteria of "responsible" civil disobedience can be identified with one or the other of two currents of thought and sometimes with both. One will be called "neo-conservative."[3] It views political obligation from a stress on the rule of law as the balance-wheel between majority will and minority rights. The rule of law includes the judicial development of civil liberties and the legislative protection of civil rights. The pattern has no internal agreement about rates and kinds of innovations

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[1] Prominent commentary on political obligation was offered by Thomas Aquinas, Locke, Rousseau, and notably T. H. Green who may have been the first to use the term. A study of Green's thought and environment is Melvin Richter, *The Politics of Conscience: T. H. Green and His Age* (Cambridge, Mass.: Harvard University Press, 1964). Reassessments include John R. Carnes, "Why Should I Obey The Law?," *Ethics,* 71 (1960), 14–26; Hanna Pitkin, "Obligation and Consent," This REVIEW, 59 (1965), 990–1000, and 60 (1966), 39–52; John Plamenatz, *Consent, Freedom and Political Obligation,* 2nd ed. (New York: Oxford University Press, 1968); T. C. Pocklington, "Protest, Resistance, and Political Obligation," a paper presented at the 1969 Annual Meeting of the American Political Science Association, New York, September 3–6, 1969; and Joseph Tussman, *Political Obligation and the Body Politic* (New York: Oxford University Press, 1960).

[2] Christian Bay, "Civil Disobedience," *International Encyclopedia of the Social Sciences,* Vol. II, pp. 473–486, at pp. 473–474.

[3] Representative statements are Francis A. Allen, "Civil Disobedience and the Legal Order," *University of Cincinnati Law Review,* 36 (1967), 1–38, 175–195; Abe Fortas, *Concerning Dissent and Civil Disobedience* (New York: The New American Library, 1968); Erwin N. Griswold, "Dissent—1968," *Tulane Law Review,* 42 (1968), 726–739; and Sidney Hook, "Social Protest and Civil Disobedience," *The Humanist,* 27 (Sept.-Dec., 1967), 157–159, 192–193.

required to strengthen democracy. Yet, it sets a premium on acceptance of the out-put of the historical constitutional system as the hallmark of the good citizen. Reflecting more than a trace of positivism, the neo-conservative tendency sharply distinguishes between private motives and legal behavior. The current is unlikely to give any significant aid to an interpretation of sovereignty whereby the citizen realizes his true morality through conformity to the state's determination of right in the tradition of Bosanquet. Lacking dedication to this notion, the neo-conservative current participates in discussions of the special conditions under which civil disobedience might take place without endangering a democratic system.[4] Its outlook, however, is often negative, aligning easily with the Kerner Report's finding that in the recent past "uncivil" disobedience shared with white terrorism and defiant officials the responsibility for creating a climate that encouraged and approved violent protest.[5]

The neo-conservative current sometimes approves or condones civil disobedience, especially to test the constitutionality of statutes, but it contrasts with a second current which welcomes the incorporation of responsible law-breaking into democratic theory as a beneficial, though rarely used, mechanism. This second alignment will be called "institutional libertarian" or simply "libertarian."[6] Ranging from the idioms of welfare capitalism to social democracy, it combines a defense of a legal democratic order with sponsorship of evolutionary changes in the distribution and uses of power to better meet human needs. Divided about a number of issues, the current has yet to show if its future lies with those concerned with perpetuating the due process tradition who attempt to define civil disobedience, such as Morris Keeton, or with "radical liberals" who are more interested in urging newer versions of democracy than in explaining how to achieve them.[7] The libertarian persuasion often serves as an infrequently acknowledged ally of black power and radical New Left groups in their "anti-oligarchy" struggles. The relationship is not one whereby these groups offer substantial analysis of civil disobedience. For the black power advocates this condition may stem from an emphasis within the early civil rights movement, and certainly from without, on its obligation to use non-violent means. The older stress invalidates discussion of civil disobedience for newer black leaders who believe, as academicians came to believe about the anti-communist provision of the National Defense Act, that a minority community had been affronted, and it would no longer tolerate the condition.[8] An explanation of why the radical New Left has not contributed to discussion of civil disobedience is that it tends to consider the phenomenon as little more than a mechanical strategy in the struggle against the alleged evils of the American and associated systems, causing a dearth of analysis according to values other than those of the crucible. The relative silence of the radical New Left about the philosophical significance of civil disobedience may be valid testimony that the subject is indeed worthless.[9]

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[4] There are commentators whose alarm about civil disobedience suggests that there is a fully conservative category. See Morris I. Liebman, "Civil Disobedience—A Threat to Our Law Society," John W. Riehm, "Civil Disobedience—A Definition," *American Criminal Law Quarterly,* 3 (1964), 21–26, 11–15; and former Justice Whittaker's remarks in Charles E. Whittaker and William Sloane Coffin, Jr., *Law, Order and Civil Disobedience* (Washington: American Enterprise Institute for Public Policy Research, 1967), pp. 1–25. A majority of the National Commission on the Causes and Prevention of Violence held that civil disobedience, including non-violent action, is potential anarchy: *New York Times,* Dec. 9, 1969, pp. 1, 44.

[5] *Report of the National Advisory Commission on Civil Disorder* (Washington: Government Printing Office, 1968), p. 92.

[6] Internal agreements on justification, and means and ends of ethical resistance provide considerable diversity to the current. But consult Hugo A. Bedau, "On Civil Disobedience," *The Journal of Philosophy,* 58 (1961), 653–665; Harrop A. Freeman and Bayard Rustin in Harrop A. Freeman, ed., *Civil Disobedience* (Santa Barbara: Center for the Study of Democratic Institutions, 1966), pp. 2–10, 10–13; Morris Keeton, "The Morality of Civil Disobedience," *Texas Law Review,* 43 (1965), 507–525; and Michael Walzer, "The Obligation to Disobey," *Ethics,* 77 (1967), 163–175.

[7] Civil disobedience is one, undefined form among many "disorderly surrogates" for socially acceptable types of protest in the stockpile of the "politics of radical pressure," recommended in Arnold S. Kaufman, *The Radical Liberal* (New York: Atherton Press, 1968), pp. 56–75, at 70.

[8] The anti-patrician and self-determinist spirit is evident in Floyd B. Barbour (ed.), *The Black Power Revolt* (Boston: Sargent, 1968), and Nathan Wright, Jr., *Black Power and Urban Unrest* (New York: Hawthorn Books, 1967).

[9] It is true that Herbert Marcuse has referred to "uncivil" disobedience. See *An Essay on Liberation* (Boston: Beacon Press, 1969), pp. 68–69. But his view of the phenomenon is controlled by beliefs that capitalist "pseudo-democracy" eventually absorbs any kind of non-mass opposition and hypocritically distracts attention from its own brutality through discovery of "illegitimate" re-

But since this paper's presumption is that the topic deserves study, it will have to proceed. Because black power and the radical New Left are not substantively concerned with civil disobedience, non-conservatives and libertarians are challenged to demonstrate that it is significantly involved in those broad issues of defining and reaching a better political life that energize non-debaters as well as many discussants.

Without pausing here to identify the specific points where the neo-conservative and libertarian currents agree or differ on the standards for "responsible" civil disobedience, I find that together they suggest the following criteria: 1) The act must be performed openly—secrecy is prohibited. 2) It must be a deliberate, not an accidental step. 3) The action is clearly unlawful, i.e., not permissible under existing laws and court interpretations of civil rights and liberties. 4) The illegal act is voluntary, not induced by others. 5) The conduct proceeds from "conscientious" dissent, inspired by moral or religious beliefs. 6) The objective sought is a concrete, public reform. 7) Legal remedies must be exhausted before disobedience is undertaken. 8) The disobedient is obligated to use "non-violent" means. 9) Throughout his challenge he demonstrates concern for the rights of others. 10) A proximate relation exists between the rule under attack and the reasons for dissent. 11) The disobedient must submit to the legal consequences of his act. This is not an exhaustive list, but it is suggestive of numerous criteria.

One can immediately comment on 1), 2) and 3). Meant to exclude subversion or evasion of law, the public test is open to a claim of exemption for some who violated the Fugitive Slave Law of 1850, the 18th Amendment and current state laws on abortions. If clandestine infractions are acceptable, perhaps only retrospective sorting by disinterested or "winning" historians can judge the disobedients. Meanwhile, one can reasonably expect that generally the public test must be met. The criterion of deliberate infraction is troublesome when the public standard is forgiven, thus making proof difficult. To consider an act eligible for "responsible" civil disobedience in the absence of evidence of advertancy, one may have to assume, intending no entrapment, its deliberate commission. On the test of illegality, there would seem to be no great difficulty in accepting the standard. A problem is how to treat the opinion that civil disobedience is not involved when the breaking of a law is subsequently found to be no violation because the "law" was invalid under superior legislation or a constitutional ruling.[10] To accept this view narrowly limits "civil disobedience" to unsuccessful, legal challenges and denigrates political and ethical justification of disobedience. This is an inhibiting prospect for political philosophy.

## II. THE ISSUE OF "CONSCIENTIOUS" OBJECTION

On the assumption that the disobedient act must be public, deliberate and illegal, this paper will examine four tests of civil disobedience which are crucial for distinguishing it from other kinds of political action and exploring its political ethics. They are the criteria of "conscientious" objection, willingness to accept the legal consequences of disobedience, "non-violent" means, and a proximate relation between the target and the grievance. To take the first, the stipulation that the infraction must be based on moral or religious conviction, i.e., the dictates of one's conscience, is found among both neo-conservatives and libertarians. Although definitions are seldom offered, both groups tend to consider "conscience" as a sacred and sovereign monitor, operating as if in the presence of God, rather than to imply Freud's super-ego, a product of fear and guilt. Moreover, they tend to think of conscience responding to significant, public challenges.[11] Neo-con-

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sistance. *Ibid.*, pp. 64–65, 76–77. A radical New Left view of the national and world scene is Carl Oglesby's section in Carl Oglesby and Richard Schaull, *Containment and Change* (London: Collier-Macmillan, 1967), pp. 3–176. For a glimpse of strategy in Students for a Democratic Society targeting, see "The Rudd October Proposals," in Cox Commission Report, *Crisis At Columbia* (New York: Random House, 1968), Appendix B. The sources, ideas and literature of the New Left in Europe and the United States are reviewed in Rosemary Ruether, "The New Left: Revolutionaries After the Fall of the Revolution," *Soundings*, 51 (1969), 245–263.

[10] Legalists associated with federal civil rights activities have argued that civil disobedience is not present when rule-breakng is later held legal under existing Congressional legislation or Constitutional principles: William L. Taylor, "Civil Disobedience," in Donald B. King and Charles W. Quick (eds.), *Legal Aspects of the Civil Rights Movement* (Detroit: Wayne State University Press, 1965), pp. 227–235, at 228–229; and Burke Marshall, "The Protest Movement and the Law," *Virginia Law Review*, 51 (1965), 785–803, at 795–796. The focus of Taylor and Marshall is on the sit-ins in the early 1960's and the arrests that followed but never received Supreme Court approval.

[11] On the possibly quandary of the nuclear commander, see Guenter Lewy, "Superior Orders, Nuclear Warfare, and the Dictates of Conscience in the Atomic Age," this Review, 55 (1961), pp. 2–23.

servatives and libertarians are not alike on exemplification. Libertarians pay tribute to such exemplars as Gandhi and Thomas More. These men, it is held, demonstrated the qualities of mind and spirit and the political ethics which all potential rule-breakers should emulate. The neo-conservatives do not take this tack usually. A representative outlook is that of Erwin N. Griswold who, instead of valuing famous disobedients, gives recognition to the "forum of conscience" described in Chief Justice Hughes' dissent in *U.S. v. MacIntosh* as an area where "duty to a moral power higher than the state has always been maintained."[12] Having done this and acknowledged a moral right to commit civil disobedience when there is a conflict of loyalties, Griswold stresses that the act is a rare event never to be cloaked with the legitimacy of civil liberties as construed by the Supreme Court and seldom to be tolerated by the majority's legislative will. Although this rendering does not rule out innovative rule-breaking, the outlook implies that conscientious disobedience has few contributions to offer democratic theory.

In contrast, institutional libertarians find civil disobedience a potential resource. Frequently they show the influence of liberal theology and resistance ideas indebted to Roger Williams, the *Vindiciae Contra Tyrannos* and John Knox. Of special interest, these commentators provide for individual civil disobedience which the elitism of Reformation traditions normally would have excluded. The method is to interpret St. Peter's "Obey God rather than men" dictum (Acts 5:29) to deny the state's absolutism and to permit citizen disobedience by divine authority without reference to human institutions.[13] The Petrine injunction has two weaknesses which are illuminated by Leslie W. Dunbar's criticism of natural law renditions for failing to require that the disobedient must always take the burden of justifying his act. He objects to a special defect:

> This comes about when the tradition asserts that the right of conscientious disobedience represents obedience to God rather than man; thus "God justifies." That this borders on blasphemy is a truth which at least two millennia have conspired in suppressing. It is of a piece with the other too numerous manifestations of western civilization's pathetic repudiation of its own responsibility for its moral values. When we take the step of civil disobedience no presumptuous claim that "God justifies me" must be on our lips; only a plea that he do so. It is not God rather than man that we obey, but God rather than man that we seek to please, and therein lies a tremendous difference. We cannot explore this matter here, other than to note that what one *offers* to God cannot be logically evaluated.[14]

While Dunbar's larger finding is that mass black protest in the South was genuine disobedience and that it was justified, not by conscience or natural law, but by its political ends and non-violent means, his unhappiness with God's "validation" is an instructive reminder that some "conscience" justifications of disobedience raise the issue of self-certification through rationally unreachable claims, and they manipulate theological ideas. The first problem complicates the task of determining constructive rebels from their opposites. Dunbar offers an appealing solution: After the mystical disobedient offers up his act, he must appeal to men for secular judgment. Thereby two courts will have the benefit of review. On manipulating theological notions, one could say that this condition is difficult to avoid in an age when inner direction is widely respected or tolerated and those who believe in extra-human causes are experiencing revolutions. A more critical response is to suggest that Dunbar's point about pleasing instead of appealing to God implies a significant part of the "Obey God rather than man" directive that is often omitted—the possibility of God's punishment if the disobedient is acting contrary to divine will and law.[15] In *Antigone* the heroine pitted her understanding of higher law against Creon's state command, but although she perished, in an ultimate sense the Gods vindicated her and punished him. Is it unfair to argue that when a disobedient invokes the Petrine doctrine, he ought to admit that he is in principle subject to the risk that God's penalties may be visited on him, not as testimony to state sovereignty, but because the law was more in tune with God's justice than his act? I think not. For without the corollary of God's justice, the "obey God rather than men" formula allows rule-breakers who invoke it to try to have the best of the subjective world in which they presumably believe. This is a questionable procedure in terms of limiting self-certification.[16]

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[12] 283 U.S., 605, 633 (1931); Griswold, *op. cit.*, 728–738.

[13] A leading statement of this view which rests its certainty in Christ is John C. Bennett, "The Place of Civil Disobedience," *Christianity and Crisis*, 27 (Dec. 25, 1967), 299–302. Religious institutions, it has been urged, have a duty to speak and act corporately on the great issues of conscience and not limit themselves to urging individual members to speak and act: Robert McAfee Brown, *et al.*, *Vietnam: Crisis of Conscience* (New York: Association Press, 1967), pp. 62–106, at 63.

[14] "Sources of Political Rights," paper read to Southern Political Science Association, Durham, N.C., November 13, 1964. Mimeo., p. 7. Italics in the original.

[15] There is also the possibility that the Devil rather than God is the source of inspiration. The perplexing implications of this option for actor and authorities were found at least as recently as John Brown of Harpers Ferry.

A comment should be made here about recent developments in non-selective conscientious objection to compulsory military service which reveal a conflict between public policy and claimed moral impulse. Applying the protection of due process, but denying that Congress had violated the First Amendment by requiring belief in a Supreme Being, the Supreme Court in *U. S. v. Seeger* held that an objector, not an atheist, with sincere and meaningful beliefs parallel to the convictions of theists who is conscientiously opposed to participation in war in any form is qualified for exemption from combattant training and service.[17] In the 1967 Selective Service Act, Congress responded to *Seeger* by removing the Supreme Being test; but, intending to exclude Seeger-type beliefs, it retained religious training and belief as the origin of conscientious objection and kept a ban on exemption based on essentially political, sociological, or philosophical views, or a personal moral code. In 1968 the Selective Service introduced a form to review Seeger-type beliefs. *Seeger* and the changes are only slight adjustments in the government's effort to determine sincerity and uphold equity. Together with the premises of the Universal Military Training and Service Act, they do not contribute to a common understanding of "conscience," and they raise questions about the government's right to define and apply tests about highest convictions.[18]

Political obedience and the issue of conscience are joined especially in controversies about *selective* objection to military service, for which there is no statutory provision in the United States. For present purposes the importance of the controversies lies in their debate of the question: do convictions that a particular war is immoral and unjust meet the test of conscientious objection? The neo-conservative is likely to hold that selective objection is judgmental, i.e., "political," and should not be confused with traditional, i.e., "religious," conscientious objection which is a fundamental and rare phenomenon that government can and should legally tolerate. To this view sincere claims of selective objection to particular wars are worthy of respect. But they cannot be honored because they do not have the requisite "religious" quality and they clash with the idea that the government's will is paramount until changed by the representative machinery or public opinion.[19] The libertarian, however, is apt to be sympathetic to a temporal faith interpretation of conscientiousness and willing to consider selective objection as potentially legitimate and not an indefensible challenge to national defense or democratic theory.[20] The difference between the two currents on the issue of selective objection focuses attention on how far each will permit legal exemptions from the requirements of citizenship to be extended in the name of conviction. Essentially the neo-conservative will allow non-performance that does not injure the rights of others or public safety, as with the constitutional refusal of Jehovah Witnesses to salute the flag, or that does not seriously affect a public mission, as in the instance of traditional conscientious objection to bearing arms at any time. In contrast to the neo-conservative, the libertarian will take greater risks in exempting non-conformists from public affirmation of national loyalty and performance of citizen duty. The former moves with court and legislative answers, the latter anticipates favorable responses from them, making their agreement on a conscience test of civil disobedience an unlikely prospect.

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[16] An example of the self-protecting formula is found in William Sloane Coffin, Jr.'s comments in Whittaker and Coffin, *op. cit.*, pp. 29–41.

[17] 380 U.S., 163 (1965).

[18] See Christopher H. Clancy and Jonathan A. Weiss, "The Conscientious Objector Exemption: Problems in Conceptual Clarity and Constitutional Considerations," *Maine Law Review*, 17 (1963), 143–160. If upheld, the Federal District Court ruling in *U. S. v. Sisson* (297 F. Supp. 902, 1969), which found that the 1967 law unconstitutionally discriminates against non-theists, religious or not, with profound moral convictions, will move the C. O. issue closer, either to the opening of Pandora's box or the victory of the inner light, depending on one's view. Ending compulsory military service would retire the question, unless there is another major war.

[19] Fortas, *op. cit.*, pp. 51–52.

[20] On selective objection to military service, the American Civil Liberties Union has equated the genuine objector's belief with conscience that is entitled to First Amendment protection whether or not he claims to be "religious." Although no testing formula was suggested, it has held that administrative scrutiny of the objector will detect the unconscientious and discourage this means to avoid the draft. *Civil Liberties* (March, 1966). Without plebiscites, church elites have endorsed selective objection. A statement upholding conscientious resistance to military service in particular wars received the approval of most American delegates to the Fourth (1968) Assembly of the World Council of Churches. The selective objection issue is a new and ambivalent question for historical critics of all wars. See American Friends Service Committee, *The Draft?* (New York: A.F.S.C., 1968), esp. Chapter 3, "From Witness to Resistance: The New CO."

### III. WHAT OBLIGATIONS DOES THE DISOBEDIENT ASSUME?

To leave the criterion of conscientious dissent suspended between two schools is not a happy condition. However, I will let it remain there until later to consider other evidence of good will, asking about civil disobedience what T. H. Green asked of the legal obligation to obey—what external things should be expected?[21] For this purpose two standards are useful—the disobedient's willing acceptance of legal penalties and his use of "non-violent" means. For the rule-breaker to voluntarily submit to the legal consequences of his act is held, especially by neo-conservatives, to be a central proof of one's good faith and lack of criminal or revolutionary intentions.[22] It is conceded that voluntary submission obliges the state to consider that the disobedient, not the rule, should be vindicated. For many neo-conservatives the test of full submission is for the disobedient to willingly plead guilty. Even though on moral and constitutional grounds he usually opposed his own formal guilt, the example of Martin Luther King, Jr. is valued by neo-conservatives because of his sacrificial style and testimony to the ideal of law expressed in this way: "And I submit that the individual who disobeys the law, whose conscience tells him it is unjust and who is willing to accept the penalty by staying in jail until that law is altered, is expressing at the moment the very highest respect for the law."[23]

On the question of submission, libertarians are likely to refer to the varieties of legal situations under which civil disobedience might take place and to the possible weakening of protections for the accused through guilty pleas. The two patterns agree on minimum requirements that the disobedient should not become a fugitive from justice and after arrest should use conventional, legal means.

Protest circles have produced criticisms or rejections of the submission criterion, especially the neo-conservative version. Notable arguments hold that only obscurantist or craven men accept the requirement; by their courageous disobedience to reform democracy for philosophical reasons, a few men have prepaid society for any inconvenience they may have caused; acceptance of punishment encourages the state to keep and enforce the law or policy under attack; not to resist imprisonment or other penalty is to discredit the logic of disobedience; the law disobeyed is clearly unjust or unconstitutional so that acceptance of punishment is testimony to moral or legal falsehood. Howard Zinn's censure of neo-conservative submission covers some of these notions and adds others:

> If a specific act of civil disobedience is a morally justifiable act of protest, then the jailing of those engaged in that act is immoral and should be opposed, contested to the very end. The protestor need be no more willing to accept the rule of punishment than to accept the rule he broke. There may be many times when protestors *choose* to go to jail, as a way of continuing their protest, as a way of reminding their countrymen of injustice. But that is different than the notion that they *must* go to jail as part of a rule connected with civil disobedience. The key point is that the spirit of protest should be maintained all the way, whether it is done by remaining in jail, or by evading it. To accept jail penitently as an accession to "the rules" is to switch suddenly to a spirit of subservience, to demean the seriousness of the protest.[24]

A radicalization of protest idiom is clearly evident in many of these ideas. Conceivably they suggest an understanding of civil disobedience that calls into doubt the foundations of the democratic state, as known to date. But even granting the presence of revolutionary ideas, the disciplined observer should not be hasty to conclude that conspiracy is at work and the commonwealth is imperiled. On the grounds that civil disobedience is a relief valve for an uncertain mixture of insurgent thought and non-revolutionary opinion, some concessions to the criticisms of the submission test should be made. In particular, the neo-conservative insistence on a guilty plea should be eliminated. The result would be to make the submission criterion no more and no less than the requirement of a *legal* struggle *within* the state's jurisdiction. A search for "sanctuary" or flight to avoid prosecution would be evidence of something other than civil disobedience.

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[21] Thomas Hill Green, *Lectures on the Principles of Political Obligation* (London: Longmans, Green and Co., 1921), pp. 35–38.

[22] Sidney Hook, *The Paradoxes of Freedom* (Berkeley and Los Angeles, University of California Press, 1967), pp. 116–118; Allen, *op. cit.*, 10–11; Fortas, *op. cit.*, p. 34. See also "A Declaration of Confidence in Columbia's Future," signed by faculty members of the Columbia Law School, *New York Times*, May 17, 1968, p. 41. Neo-conservatives honor Socrates as the Responsible Disobedient, holding that he drank the hemlock to testify to the state's integrity and the rule of law even as he believed that he had been unjustly accused and convicted. A variation is that the Gadfly's submission proved that he would let others disobey laws if they would assume the risk he had assumed: Charles Fried, "Moral Causation," *Harvard Law Review*, 77 (1964), 1258–1270, at 1269.

[23] Martin Luther King, Jr., "Love, Law and Civil Disobedience," *New South*, 16 (1961), 3–11, at 8. It is less clear that King's adherence to the edifice of the law prevailed in his last few years when war policies and the maldistribution of wealth became his targets.

[24] Howard Zinn, *Disobedience and Disorder: Nine Fallacies on Law and Order* (New York: Vintage Books, 1968), pp. 120–121. Italics in the original.

There is a close relationship between the submission test and the question of "non-violent" means. Christian Bay's encyclopedia article states that civil disobedience requires "carefully chosen and legitimate means," but holds that they do not have to be "non-violent."[25] Yet, neo-conservatives and institutional libertarians usually agree on the need for this criterion which is meant to certify the actor's acceptance of the legal structure and to help the act's efficacy by avoiding society's fear of violence and preventing counter-force. There are vexing problems of definition. Criteria-makers have come to no agreement as they face "non-violent" conduct ranging from Mennonite non-resistance through attempts to block a submarine launching to self-immolation. A main source of difficulty is the fact that efforts to expound on the moral or practical value of non-violence, efforts which have a permanent place in American social and intellectual history, have not completed the task of developing a theory of political institutions based on the norm.[26]

The problems of defining and institutionalizing "non-violence" imply a discussion of its opposite. The key question here is who may use violence legitimately? Drawing on traditional political obligation, the neo-conservative has answered with a moderate but explicit doctrine of state sovereignty. The libertarian is not greatly different, holding that the state alone has a legitimate monopoly over violence. While one could reasonably argue that the state has only a *de facto* and not a *de jure* monopoly over violence until the principle of consent is more fully realized, I see no viable alternative to retaining the neo-conservative, and by and large the libertarian insistence, that violence is rightly employed only by legal authorities who through legislative and judicial means determine what is and what is not anti-social behavior. In the crucible, public executives and their agents may equate illegality with violence and anarchism and act repressively on the confusion. The disobedient can help to prevent these defects by his peaceful conduct. More explicitly, the disobedient should be governed by the moral conviction that he is a witness to charity that he finds lacking in the state, obligating him to abstain from physical injury to persons and things. To hold that there ought to be a distinction between the lesser evil of violence to things and the greater evil of injury to persons is to invite a further debate about arson and theft, a process that will undermine the original ethic that was invoked.[27] "No contusions or breakage" may have little claim as a sophisticated rule for civil disobedience, but it is readily comprehensible and testifies to a prime democratic value, peaceful change.

A discussion of civil disobedience as potential revolution is suggested by the last criterion to be examined, a proximate relation between the violated rule and the cause for grievance. The neo-conservative tendency interprets the standard as insisting on a cause and effect bond. It is unsettled about any other understanding. Francis A. Allen writes:

> But dilemmas arise when the object of protest is a condition that does not result from the enforcement of any particular law or from the conduct of any readily identifiable person or group, but

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[25] Bay, *op. cit.*, p. 474.

[26] Commenting from within the peace movement on the division between the non-violent ethic and politics, A. J. Muste observed: "And since, in itself pacifism does not provide criteria for political discrimination, these criteria must be found elsewhere. In their search for sound criteria not all pacifists mine the same political quarry." Quoted in James A. Finn, *Protest: Pacifism and Politics* (New York: Random House, 1968), p. 200. Pacifist and related writings from William Penn to Bayard Rustin are collected in Staughton Lynd (ed.), *Non-violence in America* (Indianapolis: Bobbs Merrill, 1966). Gandhian resistance to reform the law is urged in Harris Wofford, Jr., "Non-Violence and The Law," *Journal of Religious Thought,* 15 (1957), 25–36. *Satyagraha* is appraised in my "Toward a Reassessment of Gandhi's Political Thought," *Western Political Quarterly,* 16 (1963), 99–108.

[27] Contrast Zinn, *op. cit.,* p. 46. For a dualistic thesis that when avenues for peaceful change have been closed, exhausted or found ineffective, violence is needed as much as peaceful civil disobedience as a mechanism to advance democratic values, subject to the government's enforcement of order and its removal of the causes of the outbursts, see Ralph W. Conant, "Rioting, Insurrection and Civil Disobedience," *The American Scholar,* 37 (1968), 420–433, at 433.

> which, on the contrary is the consequence of a whole complex of social, cultural, and historical factors . . . Direct action that assumes the characteristics of a secondary or tertiary boycott is not well calculated to a call for the moral response from the larger community upon which the classic theory of civil disobedience largely relies. Moreover, such programs contain a large and unmistakable ingredient of irrationality . . . Such forms of protest, whatever the provocations that induced them, represent a retreat from reason and ultimately threaten the nonviolent character of the present movement.[28]

Although Allen's illustration of a non-proximate condition is Negro disobedience for the proclaimed goal of increased economic opportunities, his comment is representative of neo-conservative concern about civil disobedience launched recently against national military policies. The libertarian tendency, which is equally devoted to rationality and peaceful means, does not share the neo-conservative objections to secondary and thus "political" relationships. To this view, Thoreau's refusal to pay his poll tax to draw attention to his objections to the Mexican War and slavery had no logical defects. Similarly, it is unlikely to fault the logic of the 1967 March on the Pentagon to publicize and alter Vietnam policies, however the means and the grievance are themselves judged.[29] Unlike the neo-conservative pattern, the libertarian outlook construes broadly the relevancy of a disobedient act for the rule broken. Although not all libertarians may object to *U. S. v. O'Brien's* denial of immunity for acts of "symbolic speech" which protested the wisdom of distant governmental decisions by challenging their legal armor,[30] the tendency they represent is fundamentally tolerant about the secondary character of rule-breaking which aims at the Leviathan through one of its peripheral laws.

Civil disobedience should not have to meet the neo-conservative exclusion of a secondary relationship between the violated law and the basic grievance. The breaking of a marginal, innocuous statute to communicate protest about a legally distant wrong is either individual witnessing or interest group behavior that a self-confident democracy can withstand without serious injury, especially in view of the public obloquy visited by majority opinion on non-conformists. If civil disobedience is restricted to the testing of rules believed unjust or unconstitutional—as the neo-conservative test of proximate relation between object and complaint requiries—civil disobedience would be confined to an essentially quasi-legal function when it may have a legitimate, politicized role as a "Question Time" for the majority will transcribed through the existing representative system. Under the heading of proximate relationship, it would seem sufficient to ask and expect an affirmative answer to the question, "Is the protest directed toward a specific need or wrong, clearly identified among the protesters; and has care been exercised to communicate its nature to bystanders and opponents?"[31] An "immoral" war or "retrogressive" tax policy would seem to be sufficiently specific; "poverty" or "racism" would probably be too general to qualify.

It would be premature to conclude that the norms of concreteness and honest publicity are enough to convince those who ask for a proximate relationship between the infraction and the grievance that the prospect of revolution is thereby significantly lessened. Indeed, the question whether revolution is or is not immediately beyond the threshold of civil disobedience can be raised at several turns in the discussion of standards for acceptable rule-breaking. Opinion falls between two limits. One is identifiable with conservativism and holds that civil disobedience is incipient rebellion.[32] The other is on the outer frontier of institutional libertarianism and asks that civil disobedience as "non-violent revolution" should be given legal immunity.[33] Although the bulk of the commentators are willing to avoid bracketing civil disobedience with revolution, neo-conservatives are apt to entertain and answer forebodingly the question: "Is there an attack on the system?" This phenomenon implies their general misgiving about allowing any rule-breaking in an operating, if imperfect, pluralist democracy. Libertarians are less fearful about civil disobedience being or becoming an attempt to displace the current regime. They depend on the standards of disclosure and "non-violence" to build a wall around the disobedient to tell him from the insurgent.

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[28] Allen, *op. cit.*, 12–13.

[29] The preparations and conduct of the October, 1967 demonstrations at the Pentagon had factors of interest to the student of civil disobedience. One participant-observer finds the criteria of advance notice, "non-violence," and appeal to the public conscience. He also suggests as the claim that the government was obliged to negotiate on how it would be disobeyed. See Norman Mailer, *The Armies of the Night* (New York: New American Library, 1968). A situational description of the Pentagon events as a mixture of Gandhian and insurgency tactics is reported in *Liberation,* 12 (November, 1967) 3–7 (David Dellinger), 26–28 (Arthur Waskow).

[30] *U. S. v. O'Brien,* 88 S. Ct. 1673 (1968).

[31] Keeton, *op. cit.,* 515. Italics of the original removed.

[32] Riehm, *op. cit.,* 14. See also Lewis H. Van Dusen, Jr., "Civil Disobedience: Destroyer of Democracy," *American Bar Association Journal,* 55 (Feb., 1969), 123–126.

[33] Freeman, *op. cit.,* pp. 5–6.

The neo-conservative fear that rebellion is inherent in rule-breaking is especially troublesome for the task of establishing criteria for civil disobedience. If the concern becomes overriding there is a risk that under its weight other criteria will fall. To avoid this contingency the discerning student will have to do one of two things. Either he will try to work out his own set of answers to show that there is no insuperable logical difficulty in viewing civil disobedience as a non-revolutionary question.[34] The other option is for him to rely on a belief that in a democratic context "principled" lawbreaking is worthy of ethical examination regardless of the risk that it may produce, intentionally or otherwise, the reality of political violence. I would recommend the second alternative, and support it with the addition of two widely-proposed criteria—the exhaustion of legal remedies before disobeence is undertaken and demonstrated care for substantial rights of others, e.g., First Amendment rights. The former helps to maintain the validity of redress procedures, which if discredited by non-use, may not provide adequate justice for anyone. The latter requires the disobedient to show non-ideological evidence of the humanism which he has proclaimed as his *weltanschauung.*

*Civil* disobedience therefore emerges as rule-breaking which meets certain standards. Assuming the act to be deliberate, it should be an articulated, public deed, aimed at a specific wrong and conducted peacefully with concern for others inside the state's jurisdiction after completing legal, remedial action. Selective borrowing from neo-conservative and libertarian norms opens the way to a discussion of whether there are significant tasks in democratic theory which might be performed through civil disobedience. Before that enterprise is undertaken some settlement must be made of the test of conscientious dissent.[35]

### IV. WHAT IS "CONSCIENTIOUSNESS"?

Because of the centrality of "conscientiousness" for many definitions of civil disobedience, it deserves the most careful evaluation as a possible source for justification of rule-breaking in a democratic context. The standard is open to two defective understandings. On the neo-conservative side there is the risk of sacrificing the possibly creative voice to the rule of law. Among libertarians there is the chance of the absolutism of individual conscience overcoming civic virtue and order. Even if a balance were struck between the tendencies, application would be a basic problem of the test. "There cannot be," Franz Neumann wrote, "a universally valid statement telling us when man's conscience may legitimately absolve him from obedience to the laws of the state."[36] Neumann may have been premature in closing the door on scientific determination of conscientiousness, but his comment recalls the near inscrutability of the inner light and the extreme difficulty of giving it consensual recognition. An amendment may be in order to require an affirmative response from the "conscience" of others as evidence of the disobedient's own credentials. The implications of this qualification include the transference of the testing process from the actor to his audience. This amendment has the drawbacks of opening the question of how significant followership or its absence is to judging the disobedient and recalling Rousseau's general will as a "conscience" for the whole community. The former leads to a discourse on effective leadership, not valid rule-breaking; and the latter leads to controversies about the meanings of the *Social Contract* that are unsettled two centuries after its publication. Because of these problems, the amendment does not offer a reliable way to determine the individual's merit.

Given the diversity of meanings and the many questions about recognition and application which spring up around "conscientiousness" as a guide to identifying disobedience which a more perfect democracy would permit, one is left with the choice of either eliminating the criterion from consideration as Sidney Hook has done in the name of secular humanism[37] or relying on an interdependency of the disobedient and the state that will salvage some acts of conscience from its power.[38] Recognizing significant problems in either direction that require further exploration, I would choose the second course, endorsing Professor Dworkin's thesis that the regime's discretionary authority not to prosecute is the least unsatisfactory device to recognize the existence and condone the public results of the dictates of private conscience.[39] Some will object that the solution leaves conscience the prisoner of state theory—any state theory. The complaint has reasonable grounds, but it overestimates the achievements of those who wish to realize the ideal in which no man is ever forced to act in a manner contrary to his conscience.[40] Another objection is that anti-populist favoritism is practiced through forgiveness of "principled" rule-breaking, an indulgence not to be extended to the "generality" of men—a term used by Judge Charles Wyzanski, Jr. when he accepted a man's refusal on non-theistic, conscientious grounds to be inducted for combat service in Vietnam.[41] The criticism is especially difficult to answer because it exposes the elitist character of civil disobedience to which so little attention has been paid.[42]

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[34] For this position, see Richard Wasserstrom, "Disobeying the Law," *The Journal of Philosophy,* 58 (1961), 641–653.

[35] Appeals to Nuremberg principles to justify disobedience have a close affinity to conscientious dissent. The focus of the relevant passages of the 1945 London Agreement on moral choice in the face of immoral state orders tends to assign the principles to a category of inner judgment. [The Agreement is in *Journal of International Law,* 39 (1945), Supp., 257–264.] It is debatable whether that judgment is "only" *entitled* to decide for disobedience or is *obligated* to so decide. The latter interpretation has been influential in popular resistance ideas. See Benjamin Spock, "Vietnam and Civil Disobedience," *The Humanist,* 28 (1968), 3–7, at 6. This stand was also adopted by Capt. Howard B. Levy in his military trial. *United States v. Levy,* CM416463 (Army Bd. of Rev. Aug. 29, 1968), *et cetera.* His view was an interpretation of Army Field Mannual 27-10, *The Law of Land Warfare* (1965). Yet, this document, which outlines legal responsibility for war crimes, stipulates neither entitlement nor obligation to disobey.

[36] Franz L. Neuman, *The Democratic and the Authoritarian State* (Glencoe: The Free Press, 1957), p. 158.

The main problem, however, is to avoid two extremes. To ignore individual conscience as a guide to state response to disobedience is an undesirable concession to legal positivism and philosophical relativism. To elevate conscience as a self-determined yardstick is an unwelcome step toward political atomism. A balance is possible when the non-prosecution option is accepted. It depends on the forbearing character of the American state. Less than optimal, the quality ought to be refined and strengthened; but its present condition is intrinsically valuable to help recognize and respect the dictates of private conscience.

## V. CIVIL DISOBEDIENCE AND DEMOCRATIC GOVERNMENT

With the completion of the foregoing sketch of criteria for civil disobedience that reveals certain political ethics and distinguishes it from political revolution and conventional dissent, a provisional response can now be made to the question whether resistance of this kind contributes a service to American democratic thought. A subsidiary issue deals with the possible defects of the current. From the standpoint of American political obligation the area that is most pertinent is the Lockean dispensation. According to this theory, primordial consent established a political society out of which a state emerged, probably through a second contract, as Althusius had argued more clearly than Locke. In any event, original consent entitles the state to a basic loyalty. The citizens retain a portion of their pre-civil authority to self-government through the persistence of inviolable rights and a representative system operating through majority will. It is the delegational machinery which normally keeps the state accountable to its citizens who give a few men a fiduciary power to govern. *In extremo,* citizens may legitimately use violence to remake the state when it violates the terms of its mandate. Minority discontent is no proof that the mandate is broken. Only majority sanction can legitimize the claim. Private conscience is recognized by the Lockean tradition, but it is not the basis for a right to rule-breaking.[43]

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[37] Hook contends that "conscience" is a dangerous guide for principled action, which if accepted, opens the way for totalitarians along with peace workers. See especially *Paradoxes of Freedom,* Chapter 3. His thesis is overly dependent on viewing conscience as a basis for *political theory,* whereas many who claim its guidance are no more or less than moralists who may be correct or mistaken in their judgments but who seldom offer political theories. I agree with Hook's evaluation that individual conscience alone is an untrustworthy basis for consent to law. Contrast Harold J. Laski, *Authority in the Modern State* (New Haven: Yale University Press, 1927), pp. 32–47. Recent controversies have caused constitutional libertarians to attempt to combine political respect for and legal skepticism about disobedience based on highest principle: "The American Civil Liberties Union Statement on Civil Disobedience," February 1, 1969, mimeo.

[38] A third way is possible if one believes that the entire question of civil disobedience is based on what may be *owed to others* through shared values and associations. For this approach, which differs from mine, see Walzer, *op. cit., passim.*

[39] Ronald Dworkin, "On Not Prosecuting Civil Disobedience," *New York Review of Books,* 10 (1968), 14–21.

[40] The ideal has received notable support from the Roman Catholic Church in Vatican Council II's "Declaration on Religious Freedom." A state, even one that is democratic, can make a declaration of this kind only at its peril. No longer a state, but still a government, the Church faces numerous dilemmas in combining the "no-coercion-toward-conscience-even-when-wrong" ideal with Apostolic and Papal theories. As discussed in the Encyclical *Humane Vitae,* the birth control issue is one controversy in which the difficulties are revealed. For the American Catholic Bishop's 1968 statement on the issue in which on net balance conscience "as a law unto itself" is subordinated to the Church's teaching authority, see "Human Life in Our Day," *Catholic Mind,* 66 (1968), 1-28.

[41] *U. S. v. Sisson,* 297 F. Supp. 902 (1969).

[42] For the caution that in a democratic context civil disobedience is minority rule, see John H. Schaar, *Loyalty in America* (Berkeley: University of California Press, 1957), p. 52.

The Lockean view of obedience and dissent has two main weaknesses. The fiction of voluntary membership in a political community is at best a literary metaphor or at worst a device for assuring the state consensualist value under the guise of a myth. For civil disobedience a result of the fault is to assign an awesome burden of moral justification for any illegal act to the disobedient, even when the law violated is patently inequitable. The civil disobedient learns what other law-breakers have found, that he faces not only the immediate legal network, but a surrogate to which presumably he had consented in a primeval cavern. Not all evidence of this problem in American political thought lies with Lockean theory, Hobbes' single contract and Leviathan having made themselves felt despite the stronger current of the former. Still, Lockean explanation about the genesis of political obligation gives the state an unearned title to expect and exact obedience even before the stuff of the second contract, the representative system, comes into focus.

The second weakness of Lockean theory lies in its anti-individualist and conformist bent that contrasts with Locke's natural rights and social ideas. The bias shows in his insistence that majority will is needed to begin the valid overthrow of unjust rulers and in the politically undernourished place given to conscience. The influence of these beliefs on any kind of dissent is especially revealed in a condition Robert A. Dahl calls a "J-Curve" situation where the bulk of the citizens agree on many political issues. Relating this condition to political philosophy, he writes:

> A vast number of questions that might be of abstract interest to philosophers, moralists, theologians, or others who specialize in posing difficult and troublesome questions are, in any stable political system, irrelevant to politics because nearly everyone is agreed and no one can stir up much of a controversy. If a controversy does arise because of the persistence of a tiny dissenting minority, in a republic the chances are overwhelming that it will soon be settled in a way that corresponds with the view of the preponderant majority.[44]

The condition described is precisely the kind where there *ought* to be an opportunity to ask troublesome questions and to demand answers from public authorities that are innovative. In an unstable system many voices are heard and the democratic state is apt to change its policies and laws. Sympathetic to Dahl's "others," I detect a need to keep open the possibility of challenges to laws and policies, regardless of the opinion of the "preponderant majority." That this can be allowed in terms of Lockean understandings of dissent is doubtful.

It would be unwise to conclude that because Lockean ideas about obedience and dissent have drawbacks their framework should be discarded. Not only are alternatives, e.g., Hobbesian or Marcusian theories, antithetical to such democratic values as privacy and self-fulfillment, but the Lockean tradition has developed intellectual commitments to civil liberties and participatory government that are crucial for the protection and advancement of human rights and regime accountability. What might be done profitably is to adopt an idea of "justice" to link, without confusing, the rulers and the governed. Skeptical of founding "justice" on the mythical soil of contract, I agree, nonetheless, that some understanding of the notion might overcome the weaknesses of Lockean theory.[45] Understood as a trans-political, moral ideal rooted in the nature and destiny of man, "justice" partakes of T. H. Green's "common good,"[46] despite the reservations that can be introduced about his statism and unclear teleology. For civil disobedience an eclectic conception of "justice" which is not mortgaged to primordial contract or state "ends" provides a shared field for presumptions about obedience to compete with disciplined challenges. The competition is on terms which do not preordain the outcome, although they unapologetically respect systemic legitimacy.

To proceed this far is to go beyond the matter of imperfections in the predominant stream of American democratic thought and to enter a discussion of whether civil disobedience is well qualified to guard and further democratic ideals. On this question, Bay is a prominent spokesman for the affirmative. He suggests that civil disobedience can be a new and vital way to realize human values no longer served by majoritarianism because of the inroads of modern knowledge and the distortion of the rule of law and representative machinery into ultimate goods. Visualizing a non-elitist polity with a reallocation of power that lessens poverty and violence and expands human rights, he wishes to substitute for the classic justification of democracy through political obligation an ethic of individual responsibility for the likely results of one's political conduct, including obedience to law.[47] This proposal is valuable to the degree that it reminds conservative and neo-conservatives of Bracton's dictum that the king is *sub deo et lege,* and how in our time the rule of law means that "the law itself is based on respect for the supreme value of human personality."[48] Bay's reliance on individual rectitude is more controversial, for although he denies that Thoreau is the model for civil disobedience, his ideas align with the New Englander's thesis that the free man defines his own responsibility.[49] The realization of Bay's goals, which others seek, requires limits. I contend that the limits mean that the free and responsible man operates *within* a community that develops his rectitude and the state's. Is this stand close to that of David Spitz who refers to the foundation of the problem of political obligation in the Aristotelian question about whether a good man can always be a good citizen? He argues that it is possible to be true to both through the loyalty of the citizen to the principles of democracy, despite the presence of unjust laws. In the last analysis, Spitz relies on the goodness and honesty of man operating from moral principles to judge rationally the likely results of his disobedience, and to bring his pressure to bear on the state to try to produce a greater benefit than what is apt to come from obedience.[50] As an amendment, I would suggest that, in addition to combining the good man's work for the development of the state, the state must be allowed to help him. This is not testimony to the sanctity of the state.[51] As in classical liberalism, he will control the moral balance, but there is or there ought to be a mutuality between man and the state.[52] For state power can help to liberate him from deprivations. Existentialism to the contrary, he cannot perform the task alone.

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[43] A similar understanding of Lockean obligation and dissent is Harry Prosch, "Toward An Ethics of Civil Disobedience," *Ethics,* 77 (1967), 176–191, at 178–179.

[44] Robert A. Dahl, *Pluralist Democracy in the United States: Conflict and Consent* (Chicago: Rand McNally and Co., 1967), pp. 272–273.

[45] Justice and contract are joined in John Rawls, "The Justification of Civil Disobedience," in *Civil Disobedience: Theory and Practice,* Hugo Adam Bedau, ed. (New York: Pegasus, 1969), pp. 240–255.

[46] On Green's "common good," suggesting that its social context is true but because of faulty use of words he failed to distinguish different goods, see Plamenatz, *op. cit.,* pp. 62–81.

Applied to responsible civil disobedience, mutuality does not mean that legal immunity ought to be extended to it. This would be to legitimize and normalize civil protest into a formal, remedial institution that the democratic system already provides in other ways. Routinization would also destroy the logic and spontaneity of disobedience. Yet, civil disobedience can aid, as it has in the past, the selective incorporation into civil rights and liberties of public speech and behavior previously denied to citizens. While some attempts fail and may deserve to fail in such marshy areas of criminal law as conspiracy and trespass, disobedience of the law, as the neo-conservative admits, is sometimes a useful tool to expand the arena of liberty. Moreover, responsible civil disobedience can continue to aid social changes that are beyond the capacity or will of representative institutions. Although civil rights disobedience within individual states had their approval, this is a basic frontier neo-conservatives would rather not pass. They can be answered with the reply that civil rule-breaking can energize the political system to take additional steps to advance social progress, provided it is understood that the criteria suggested in this paper or similar tests are developed as insurance that principled disobedience is not a threat to the constitutional order, but an effort to retain it because of its demonstrated values.[53]

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[47] Bay, *op. cit.,* pp. 484–485. The populism inherent in Bay's ideas is more apparent than real, for he would trade the elitism behind liberal democracy for a new elitism of radicals who would produce bold policies.

[48] International Commission of Jurists, *The Rule of Law In A Free Society: A Report on the International Congress of Jurists, New Delhi: 1959* (Geneva: International Commission of Jurists, 1960), p. 196. Bay criticizes the dogma of the rule of law in pluralist democratic theory in "Civil Disobedience: Prerequisite for Democracy in Mass Society," in *Political Theory and Social Change,* David Spitz, ed. (New York: Atherton Press, 1967), pp. 163–183.

[49] "The only obligation which I have a right to assume," Thoreau said, "is to do at any time what I think right." "Civil Disobedience," *The Works of Thoreau,* Henry S. Canby, ed. (Boston: Houghton Mifflin Co., 1937), 789–808, at 790–791.

[50] David Spitz, "Democracy and the Problem of Civil Disobedience," this Review, 48 (1954), 386–403, at 401–403.

[51] On some dangers of civil religion, see Lewis Lipsitz, "If as Verba Says, the State Functions as a Religion, What are We To Do To Save Our Souls?", this Review, 62 (1968), 527–535. A vineyard note is Barbara Deming, "Desanctifying Authority," *Liberation,* 12 (November, 1967), 32–33.

[52] "Mutuality" between citizen and state is explored in Robert J. Pranger, *Action, Symbolism, and Order: The Existential Dimensions of Politics in Modern Citizenship* (Nashville: Vanderbilt University Press, 1968), pp. 54–57.

Those who believe that the representative model, operating fundamentally by "majority" will, coalition leadership and a developing system of rights and liberties, is the best arrangement to be expected in an imperfect world, are unlikely to invite civil disobedience into the house of Madison and Lincoln. Essentially, this is the neo-conservative current. Some, but perhaps not all, libertarians are open to the prospect of legitimizing civil disobedience as a genuinely democratic method. They believe, and I would agree, that civil disobedience can provide a theoretical service in a democracy through the reform of the delegational pyramid. For without contending that Rousseau's city-state ought to be the paradigm for democracy, I suggest that the representative structure which welfare democracy inherited from last century's liberalism and improved in this century has not been so administratively effective or so politically virtuous that it cannot be improved by new arrangements for combining obedience and consent. To experiment is a risk for democratic political obligation. But is there not an equally serious risk if stability is valued ahead of the expansion of accountability?[54] Distinguished from the use of force to impose decisions and from the existing regime of rights and liberties, disciplined civil disobedience is possibly a creative way to ask the citizens of the state if they are satisfied with other aspects of the delegational model that has served well but which may not have produced the most equitable and efficient allocation of power and resources to deal with emergent disaffection and unmet needs in the national polity.

But how *necessary* is civil disobedience, given the availability of many legal forms of political opposition and reform? To face this question is to appeal for empirical studies of the benefits or costs of civil disobedience undertaken in our time, measured against the reasonably well-known strengths and drawbacks of conventional forms of political opposition. Recent studies of violence in the United States are of no great value because they have proceeded almost without the aid of ethical distinctions about various kinds of disorder.[55] The distinctions, as I have tried to show, can be made. The task is to use some set to isolate *civil* disobedience from other forms of unconventional dissent and after empirical analysis to declare whether there is a *need* for this special form of opposition in terms other than the ones that are used frequently to justify it.

Meanwhile, three compelling reasons support the incorporation into American democratic philosophy of *civil* disobedience. As a buffer between civil liberties and rights, and direct action and Communardist ideas, having kinship with both, it provides a testing zone for challenges to representative democracy without complete submission to either established or new rules of the game. Civil disobedience takes soundings for the operative formulas of democracy, not the least of which is how to probe for a conception of justice held by dissidents and state alike. Finally, the phenomenon is an educational strategy to rethink persistent questions of political obligation. For all three reasons incentives are supplied, not only by intellectual curiosity, but also by the power, merits and inadequacies of discontent itself.

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[53] For this point, see Wilson Carey McWilliams, "Civil Disobedience and Contemporary Constitutionalism: the American Case," *Comparative Politics,* I (1969) 211-227, at 221.

[54] Political obligation rests on a narrow interpretation of order and stability in Charles S. Hyneman with Charles E. Gilbert, *Popular Government in America: Foundations and Principles* (New York: Atherton Press, 1968), 274–275.

[55] A minority of the Eisenhower Commission on Violence cited ethical criteria in its dissent from the majority's faulting of civil disobedience: n. 4, *supra.*

# RATIONAL BEHAVIOR IN POLITICS: EVIDENCE FROM A THREE PERSON GAME*

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## I. THE CONCEPT OF "RATIONAL BEHAVIOR"

A fundamental controversy in political theory from ancient times until the present concerns the rationality of political actors, what it is, if it exists at all, and whether or not humans display it in politics. Many political scientists are impatient with this controversy because it remains open after so much (apparently futile) discussion. But they ought not be. The problem of rationality is necessarily imbedded in even the simplest kinds of political research, where, if overlooked, it can occasion misinterpretation and even outright error.

Suppose, for example, in an investigation of legislators one uses the notion of party loyalty as an independent variable to explain behavior. This notion seems simple and straightforward enough and not, therefore, likely to involve one in philosophical controversy. But in fact party loyalty can be interpreted in a variety of ways and the choice among them necessarily involves a choice on one side of the controversy over rationality. Loyalty can be thought of, for example, as a truly independent variable, as a product of political socialization, as an expression of affect, and hence as an essentially irrational motive. On the other hand, it may be thought of as itself dependent on bargains rationally satisfying the preferences of legislators. Such bargains may be either short term or long term so that a legislator's manifest party loyalty may result from a series of advantageous bargains with party leaders on particular bills or from an implied bargain with them on career advantage. But, in either case, if party loyalty is the product of bargains, it is something quite different from loyalty based on affect. And this difference in turn makes a profound difference in interpreting the behavior of legislators for a loyalty based on bargains is subject to strains and stresses not found in a loyalty based on affect. Which kind of loyalty exists thus makes a difference in the prediction of behavior and, furthermore, the quality of prediction thus depends on a decision in the philosophical controversy.

This controversy is pervasive in political science. The controversy implicit in the notion of party loyalty as applied to legislators is equally implicit in the notion as applied to voters, and, beyond that, is implicit in such notions as class interest, national interest, alliance membership, etc., when used as variables to explain the behavior of men or nations. It is not possible, therefore, to avoid the controversy in serious political research.

Pervasive as this controversy is, political science has made remarkably little effort to resolve it for its own concerns. Although the present form of the question is: "Are men rational in making decisions?," the greater portion of the philosophizing about political rationality has been in terms of the issue as it was raised by Plato, the Stoics, and the philosophers (e.g. Hobbes and Rousseau) and lawyers (e.g. Grotius and Blackstone) of early modern times. These writers saw no issue in whether or not men are rational. All assumed men are. Rather they debated the nature of this rationality: do men, in behaving rationally, seek to maximize self-interest or to maximize some external goal as set forth for them in divine law, natural law, or the general will? An illustration of the regressive quality of much of current political philosophy is the recent volume of *Nomos* on rationality in which almost all the discussion is in these anachronistic terms.[1]

But while political philosophers keep on debating the issues raised by Hobbes and Rousseau, the controversy has changed. Now, as a result of philosophical and scientific inquiry in

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[1] Carl J. Friedrich (ed.), *Nomos VII: Rational Decision* (New York: Atherton Press, 1964). There is, however, one work which blends the old and the new very nicely, Paul Diesing, *Reason in Society* (Urbana: University of Illinois Press, 1962).

the two intervening centuries, the question has become: "When faced with the necessity of decision among several alternatives, do men order their preferences among them and choose that alternative which maximizes their utility?," where, of course, utility is understood as a measure of ordered preferences. Or, assuming a general affirmative to that question: "On the basis of what goals do men order their preferences?"[2] Around these questions today there are a variety of philosophical positions, among the most important of which are:

1. *Those which assert that choosers maximize utility.* Although the contemporary intellectual world is often described as anti-rationalist, the science of economics, which is the most practically successful and intellectually elegant of the social studies, is built entirely on the premise of rational behavior. In an anti-rationalist world, it has kept alive one side of the older rationalism (i.e., the radical individualism and nominalism of Hobbes and Bentham); but it has so refined the problem that its genetic resemblance to its ancestor is only barely visible. Its picture of the rational calculus is that men order preferences according to their tastes, although they impose on the ordering some elementary logical requirements of coherence and consistency. Once ordered, men then behave in situations involving risky choice as if they were maximizing expected utility, where utility is a numerical measure on preference orders. If one assumes that tastes are given and inscrutable, then the will appears as arbiter of taste, and reason appears as the efficient servant of the will. In such a view of rationality, which we call procedural, men are invariably rational. The only way an observer can discover the inscrutable goals is by observing choices among alternatives. From the choice, and from the assumption that the chooser maximized his utility, the observer infers the subject's goals. Although this theory is tautological, it does assert something testable, namely that men behave as if they were rational calculators. In many economic and political applications, however, one assumes further that a particular goal is appropriate for all men in the particular circumstances of the choice. In this view of rationality, which we call substantive, men as producers are said to maximize profit, men as consumers are said to minimize cost, etc. This theory, since it is not tautological, allows tests of assertions about calculation but unfortunately it also involves the scientist in attributing goals and necessarily, therefore, involves the chance of error from this source. Both versions of rationality, regardless of whether or not a goal is postulated, do, however, assume that men are rational calculators, which is what other, subsequently listed, theories do not assume.

2. *Those which assert choosers choose by habit and discovery rather than by analysis of preference.* Learning theory, which derives from that great nineteenth century realization that man is part of the animal kingdom and from the great twentieth century realization that laboratory animals (and thence man) could be taught to respond positively to stimuli associated with rewards, offers a totally different picture of choice from that implied in the question as we have stated it. In learning theory, choices are said to occur as a result of rewards and punishments administered for previous behavior. If a subject—man or rat—chooses in random exploration an alternative that results in a reward, then on subsequent presentation with the same alternatives he chooses that which led to the reward. The chooser, of course, has preferences, even ordered preferences. And in this sense learning theory is like the theory of rationality in that men are said to choose what is preferred over what is not preferred. But the theories differ in that, in learning theory, the choice results not from a calculus but from the recall of previous consequences. Naturally, there is no maximization of utility here, merely the satisfaction of some desires, which are not necessarily the most pressing ones.

3. *Those which assert that choosers do not maximize utility.* Here the most well-known position is the psychoanalytic, which ultimately derives from the exaltation of will against reason in such writers as Schopenhauer and Nietzsche. As against the rationalistic view that reason is the servant of the will, here will and reason are at war. Not unexpectedly, therefore, the very notion of ordering preferences is rejected as an unrealistic description of behavior. Instead, choice is interpreted as the product of sexual forces modified by rationalizing and socializing counter forces in an essentially disorderly way. To the psychoanalyst, therefore, the answer to the question of whether or not men choose so as to maximize utility is not simply "no" but rather that men are likely to choose exactly oppositely. Somewhat similar to the psychoanalytic position in this respect is the theory of cognitive dissonance. According to this notion, the relation of choice to preference as we stated it in our initial question is a reversal of nature. Instead of choosing so as to maximize utility (a measure of preference), men are said to construct preferences in order to rationalize

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[2] This question, while superficially resembling the old one, differs in the expectation that goals be empirically investigated rather than attributed in arm-chair dicta.

choice. What comes first is choice based on some wholly unrationalized affect such as identification or Oedipal urges or what not. If this choice is challenged, then preferences are ordered to rationalize it. Hence choice is first and preference second, an outright reversal from the theory of rational behavior. Naturally, the theory of cognitive dissonance, like psychoanalysis, has no place for choice that maximizes utility.

Such is the confrontation of rationalism and anti-rationalism as it appears today in social science. A remarkable feature of the dispute is that each of the viewpoints, while supposedly universal, is actually quite closely linked to the behavior studied. Thus the main contemporary exponents of the model of calculating man are economists, who study behavior in the market. The main exponents of the model of choice by habit and discovery are learning theorists, who study behavior in the rat-maze and the class room. The main exponents of the model of choice by reason of sexual drives are psychoanalysts, who study behavior in dreams and fantasy-making. And the notion of cognitive dissonance with its emphasis on choice as against preference was first applied in a study of religious behavior. Each theory, however, seems to work fairly well when applied to the behavior for which it was created and to appear insufficient when applied to behavior generally.

This fact creates a difficult problem for political theory. If, as we have argued, the quality of political research depends in part on the position the researcher takes on the question of rationality, he must choose among the theories in this spectrum. But to do so he needs to make some decision on the kind of behavior he is studying. Is political behavior closer to behavior in the market or to behavior in fantasy? If we could answer this question with assurance, we could adopt wholesale a theory from another discipline. But assuming politics is, in Easton's words, the authoritative allocation of values, it is uncertain whether utility theory or something quite different applies. So far as politics is allocation, it seems analogous to economics, which is often defined as the study of the allocation of scarce resources. So far as politics is authoritative, however, it seems analogous to the world where learning theory applies, with its emphasis on rewards and punishments or even to the dark and vicious world of fantasy where power is thinly-veiled sadism and authority is power with yet another veil.

Given this ambiguous nature of politics, it seems desirable, therefore, to investigate action in a political setting in order to determine in some preliminary way, at least, what kind of behavior is political behavior and hence what kind of theory about rationality is appropriate for political science. That is, we need to look at characteristically political behavior with a view toward determining what kind of theory of rationality fits it best.

To a small degree, such work has been done. A generation ago Harold Lasswell studied several politicians psychoanalytically, though the relation between their behavior on the couch and their behavior in political office was never made clear.[3] Furthermore, Lasswell wrote as a proponent of a particular theory rather than as an evaluator of competing theories. More recently Robert Lane has studied by depth interviews the thought processes on political issues of a few ordinary citizens, although again the relation of their attitudes to their political actions was not investigated.[4] Much of the early survey research on voting by Paul Lazarsfeld and, especially, Bernard Berelson was interpreted by its authors in anti-rationalist terms, thereby setting something of a style for subsequent writers; but V. O. Key responded in *The Responsible Electorate* with what reads very much like an interpretation in terms of utility theory of the whole range of data from survey research about voting.[5] Recently Peter Ordeshook and the senior author attempted to interpret data on the decision to vote so that behavior followed directly from the assumptions of utility theory, although one term of the equation looks very much as if it originated in learning theory.[6] Although Herbert Simon is responsible for introducing political scientists to learning theory (through his notion of "satisficing" as contrasted with "maximizing"), he never really used it to investigate political behavior. Others, however, have, especially Charles Lindblom, with his notion of incrementalism which is derived from Simon's "satisficing" and which is offered as a general rule of political decision.[7] And

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[3] Harold Lasswell, *Psychopathology and Politics* (Chicago: University of Chicago Press, 1930).

[4] Robert Lane, *Political Ideology* (Glencoe: The Free Press, 1962).

[5] Paul Lazarsfeld, Bernard Berelson, and Hazel Gaudet, *The People's Choice* (N.Y.: Duell, Sloan, and Pearce, 1944). Bernard Berelson, Paul Lazarsfeld, and William McPhee, *Voting* (Chicago: University of Chicago Press, 1954). V. O. Key, *The Responsible Electorate* (Cambridge: Belknap Press of Harvard University Press, 1966).

[6] William H. Riker and Peter Ordeshook, "A Theory of the Calculus of Voting," this REVIEW, 62 (1968), 25–42.

[7] David Braybrooke and Charles Lindblom, *Strategy of Decision* (Glencoe: The Free Press, 1963).

Richard Fenno and Davis, Dempster, and Wildavsky have applied something of this notion to explain appropriations decisions.[8] Unfortunately this short survey mentions most of the important work that has been done. And except for the work by Key none of it has been undertaken in a mood of evaluation and for the purpose of offering an answer to the question of what kind of theory about rationality is appropriate for political science. Clearly more diverse investigations than these are desirable in order to clarify this question. In this essay, therefore, we offer a report on one such investigation which suggests that political behavior is the kind of behavior for which utility theory is appropriate.

## II. THE GAME SETTING

It would be fortunate if we could study rationality in politics directly—and sometimes we can as when we investigate whether or not people vote in accord with previously expressed preference. But most political activity, especially that most interesting kind involving the work of professional makers of majorities and public policy, is not readily open to direct investigation. Consequently, we have used surrogate political subjects in a surrogate political setting and we offer evidence about their behavior as indirect evidence about political behavior in a wider world.

This study is based on the assumption that what happens in n-person games is closely analogous to what happens in politics. The main activity in an n-person game is making coalitions (that is, making policies that satisfy a majority), which is what political leaders spend most of their time doing. Political followers, when they act politically (as in voting), are mainly engaged in joining coalitions. Hence the game activity of making coalitions is very much like the essential activity of politics.

Games are not, of course, perfect analogies for unabstracted natural events, as indeed are not any other laboratory experiments in any social or natural science; but they are closer analogies for politics than any others yet offered. Surely they are closer to the characteristic action of politics than fantacizing on a psychoanalyst's couch. At least games involve those interpersonal transactions which are essential to politics, while behavior in confession does not even involve that. Games, furthermore, seem to us to be better analogies than learning experiments which typically involve only problem solving and the avoidance of error. Politics does doubtless involve this, but it includes much more, especially the manipulation of relationships with others, creativity in organization, and the like. And these are precisely the kind of activities that are captured in the n-person game.

Conversely, behavior in games is probably more closely analogous to politics than is behavior in the market. Transactions in the market are by definition harmonious—at least in the absence of externalities—for every sale is a purchase and both buyer and seller invariably leave the market better off in utility. Even if no trades occur, no one is worse off. It is not so with politics. Some political situations such as elections, are zero-sum, i.e., such that the winner wins what the loser loses. And this guarantees the existence of losers, people who are worse off because they entered the political arena. Even if politics is not zero-sum, typically there are losers. Consider a civil war, say the American Civil War, which may have been zero-sum with respect to the governments but was surely not so with respect to the participants. The losers of that war precipitated it to protect (and extend) slavery; but what they got for their trouble was the Thirteenth Amendment. The war was not zero-sum because the winners did not get the slaves the losers lost. But the losers surely lost something and from hindsight we know they would have been wiser not to have fought at all. So it is with much of politics. Entry into the political arena necessarily involves the risk of loss which is not fully captured by market analogies. Game analogies, however, capture this feature of the real world.

There are, of course, many points at which the game analogy fails. One can try in the laboratory to introduce some of the features of the political world, but the game remains an analogy, not the real stuff of politics. In this experiment we have tried to compensate in a few ways not likely to occur to psychologists for the fact that the laboratory is not the wider world. For example, our subjects were self-selected (that is, they answered advertisements) and were allowed to drop out if they didn't like the game (usually because they lost), just as politicians are self-selected and remain or drop out by a kind of natural selection of winners. For another example, in some of the experiments the subjects played against each other several times and knew from the beginning that this would be so, just as politicians know and are constrained by the fact that they expect to meet again. (From the protocols, we know our subjects were conscious of the repetition, but we suspect that they were not nearly so preoccupied with it as politicians are said to be.) Again, our subjects, while initially unacquainted, saw each other in the laboratory often enough in intense situations to develop strong feelings of friendship and enmity, which we interpreted as the laboratory equivalent of loyalty, hostility, and even ideology in the political world. But we know, of course, that these putative equivalents are pretty pale imitations of these forces in political life. So, although we have tried to fit the laboratory circumstance to the political circumstance, we know we have not been wholly successful and the game remains only an analogy of politics, not politics itself. But until we can get real politicians to answer questions they would surely regard as silly, we think games in the laboratory are about the best we can do to study political behavior in exhaustive detail.

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[8] Richard Fenno, *The Power of the Purse* (Boston: Little Brown and Co., 1966). Otto Davis, M. A. H. Dempster, and Aaron Wildavsky, "A Theory of the Budgetary Process," this REVIEW, 60 (1964), 529-547.

## III. FEATURES OF THE GAME

Although the senior author has elsewhere described this game in some detail, we repeat a brief description of its major features:[9]

1. There are three players. In most groups of subjects these have been college undergraduates, although one group consisted of businessmen. All were from middle class backgrounds. Subjects were assigned to all three positions so that each subject played each position in about one-third of the matches he played.
2. The players negotiate in pairs about coalitions, usually thus:
   (1,2) for five minutes, while 3 is in another room.
   (1,3) for five minutes, while 2 is in another room.
   (2,3) for five minutes, while 1 is in another room.
   This order is then repeated for three (or in some cases, three to six) more conversations. With two groups the order of conversations was varied.
3. At the end of negotiations, players are asked privately and individually what coalition they have formed. If two agree, they are paid, otherwise not.
4. Payments are according to the following schedule:
   if (1,2) forms, it receives $4.00
   if (1,3) forms, it receives $5.00
   if (2,3) forms, it receives $6.00
   if (1), (2), (3), or (1,2,3,) form, they receive nothing.
5. The von Neumann-Morgenstern solution to this game is that one of the following divisions of the stakes occur (the "principal points"):

| Player 1 | Player 2 | Player 3 |
|---|---|---|
| 1.50 | 2.50 | 0 |
| 1.50 | 0 | 3.40 |
| 0 | 2.50 | 3.50 |

or that any one of an infinity of divisions occur, having the properties either:

1) $1.50 \leq x_1 < 3.50,\ 2.50 \leq x_2 < 4.50,\ 0 < x_3 < 2.00,$ and

$$\sum_{i=1}^{3} x_i = 6.00$$

2) $1.50 \leq x_1 < 2.50,\ 0 < x_2 < 1.00,\ 3.50 \leq x_3 < 4.50$ and

$$\sum_{i=1}^{3} x_i = 5.00,$$

where $x_i$ is the payment to the ith player.[10] The informal rules of this game were such that a division of the latter two sorts were hard to arrive at, although a few of our ingenious subjects found ways to get to them.

## IV. INITIAL FINDINGS

As we interpreted the behavior of the initial group of subjects, it became apparent that they were behaving rationally in some profound and not immediately obvious sense. Experimentation was started with a game which had a solution of [(0,2.00,0), (0,0,4.00), (0,2.00,4.00)] but then was changed to the present game because the subjects found the original game very boring. It was not until this first group had nearly completed its matches with the present game that we realized that they were averaging something quite close to the von Neumann-Morgenstern solution, which is derived directly from utility theory and some elementary standards of rational behavior. That these subjects in these circumstances should, fairly consistently, come close to a socially rational solution seemed to us compelling evidence that utility theory was an appropriate description of their behavior. It seemed even more compelling from the fact that both the subjects and the experimenters were in effect unsophisticated about the solution.

Subsequent groups of subjects further and strongly confirmed our belief by coming as close or closer to the solution, although in some groups some of the subjects were initially sophisticated (either because they had solved the game immediately upon learning the rules or because they had learned the solution from subjects in previous groups) and communicated their knowledge among the other subjects as the matches progressed for several weeks. Of the seven groups reported on here, the first, fourth, and seventh were initially unsophisticated and the first and seventh remained so to the end. Since sophisticated and unsophisticated subjects behaved about the same way in arriving at outcomes and since we know from observation and protocols that sophisticated subjects seldom let their knowledge hinder them from pressing a bargaining advantage or sadly recognizing a disadvantage, the degree of sophistication appears to be irrelevant. On the other hand, in their consistent propensity, sophisticated or not, to come close to the solution, each group of subjects further confirmed the belief that they were in sum and on the whole behaving rationally. Even in the five matches in group six where three-person coalitions were formed, the divisions were within the solution and hence quite rational.

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[9] William H. Riker, "Bargaining in a Three Person Game," this REVIEW, 61 (1967), 642–656.

[10] John von Neumann and Oskar Morgenstern, *The Theory of Games and Economic Behavior* (Princeton: Princeton University Press, 1953, 3rd edition), pp. 403–418. A simple exposition of the derivation of this solution from the notions of utility theory can be found in R. D. Luce and Howard Raiffa, *Games and Decisions* (New York: Wiley, 1957), pp. 200–203, and William H. Riker, "Bargaining in a Three Person Game," *op. cit.*

In Table 1 is recorded a summary of the results of 206 matches in seven groups. The actual outcomes of each match are reported elsewhere.[11] For each two-person coalition, Table 1 records in the first row the number of times this coalition occurred. (Matches in which only single-member coalitions occurred—16 in all—are omitted as are the 5 matches where three-person coalitions were formed.) In the second row is the mean amount ($\hat{\mu}$) earned by the player in the designated position, while the third row contains the standard deviation ($\hat{\sigma}$) around the mean. (Since we have treated these as samples out of a population of possible matches, we have put hats over $\hat{\mu}$ and $\hat{\sigma}$ to indicate they are sample estimates, in the case of $\sigma$ an unbiassed estimate.) In the fourth row is the $t$ statistic and in the fifth row in the probability that a $t$ statistic of that absolute value might be achieved by chance.

The information in Table 1 permits us to observe how very close the subjects often came to the von Neumann-Morgenstern solution. The null hypothesis is that the mean payoff to the players is, for 1, \$1.50, for 2, \$2.50, and for 3, \$3.50. The probabilities of the occurrence of $t$'s of the particular absolute value (since this is a two-tailed test) indicate whether or not the null hypothesis can be rejected. From these probabilities, it is clearly apparent that the null hypothesis cannot be rejected, except possibly in the case of coalition (2,3) for group 4.[12] All other $t$'s are well within the range of chance occurrence. The inference then is that in all but, perhaps, this one case, the variations around \$1.50, \$2.50, and \$3.50 are insignificant random variations showing no particular direction or amount of deviations. Furthermore, one also infers that, whether the subjects knew it or not, they were coming close to the solution and hence were acting as if to secure the predicted dollar rewards.

Nevertheless, while the net results of transactions as expressed in the divisions appeared clearly to result from rational behavior, paradoxically the individual behavior did not in itself appear to be rational in the sense of maximizing utility. Often subjects voted (and usually thereby chose between two alternative coalitions) in such a way as to reject the coalition that offered them the largest absolute value. For example, player 3 might be offered \$3.80 out of \$5.00 by player 1 and \$3.45 out of \$6.00 by player 2—and yet might choose (2,3) and \$3.45 instead of (1,3) and \$3.80. On a simple interpretation of maximization, clearly such a player was not maximizing. Still his behavior on the average turned out to be very close to what an abstract maximizer is calculated to bring

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[11] For groups one to three, see Riker, "Bargaining in a Three-Person Game;" for groups four to seven, see Riker, "An Experimental Interpretation of Formal and Informal Rules of a Three Person Game," in Bernhardt Lieberman, *Social Choice* (forthcoming).

[12] By reason of another consideration this one case in which rejection of the null hypothesis is feasible does not invalidate the inference because the exception has a special explanation. See William H. Riker, "Experimental Interpretation. . . ." In groups 1–3, 5, and 6, the order of negotiations was [(1,2), (1,3), (2,3)] so that player 1 was omitted from the last certain conversation. For group 7, the order was [(2,3), (1,2), (1,3)], so that 2 was omitted, while for group 4, it was [(2,3), (1,3), (1,2)], so that 3 was omitted. The effects of omission were, for 1 and 2, that they simply did not win as frequently as might be expected although they won the expected amounts in each match. Player 3, however, won just about as frequently when omitted, but won significantly less money. Recognizing that this variation in outcome was thus occasioned by the informal rule that brought the mathematical game into temporal reality, we can then understand that this one deviation in the outcomes reflects not a failure to achieve the rational solution but rather a rational adjustment of the solution in response to a special rule.

TABLE 1. SUMMARY OF OUTCOMES OF MATCHES BY GROUPS*

| | Coalition (1, 2) | | Coalition (1, 3) | | Coalition (2, 3) | |
|---|---|---|---|---|---|---|
| | 1 | 2 | 1 | 3 | 2 | 3 |
| *Group 1* | | | | | | |
| $n$ | 9 | | 10 | | 13 | |
| $\hat{\mu}$ | 1.69 | 2.31 | 1.42 | 3.58 | 2.52 | 3.48 |
| $\hat{\sigma}$ | .349 | .349 | .307 | .307 | .360 | .360 |
| $t$ | .54 | | .25 | | .05 | |
| $Pr[t]$ | $P[\lvert t\rvert > .54] \simeq .60$ | | $P[\lvert t\rvert > .25] \simeq .80$ | | $P[\lvert t\rvert > .25] \simeq .96$ | |
| *Group 2* | | | | | | |
| $n$ | 4 | | 3 | | 12 | |
| $\hat{\mu}$ | 1.56 | 2.44 | 1.62 | 3.38 | 2.47 | 3.53 |
| $\hat{\sigma}$ | .658 | .658 | .262 | .262 | .589 | .589 |
| $t$ | .09 | | .46 | | .05 | |
| $Pr[t]$ | $P[\lvert t\rvert > .09] \simeq .90$ | | $P[\lvert t\rvert > .46] \simeq .60$ | | $P[\lvert t\rvert > .05] \simeq .95$ | |
| *Group 3* | | | | | | |
| $n$ | 5 | | 15 | | 18 | |
| $\hat{\mu}$ | 1.46 | 2.54 | 1.41 | 3.59 | 2.47 | 3.53 |
| $\hat{\sigma}$ | .055 | .055 | .222 | .222 | .214 | .214 |
| $t$ | .73 | | .41 | | .14 | |
| $Pr[t]$ | $P[\lvert t\rvert > .73] \simeq .50$ | | $P[\lvert t\rvert > .41] \simeq .60$ | | $P[\lvert t\rvert > .14] \simeq .85$ | |
| *Group 4* | | | | | | |
| $n$ | 9 | | 5 | | 14 | |
| $\hat{\mu}$ | 1.44 | 2.56 | 1.77 | 3.23 | 2.88 | 3.12 |
| $\hat{\sigma}$ | .556 | .556 | .365 | .365 | .187 | .187 |
| $t$ | .10 | | .74 | | 2.03 | |
| $Pr[t]$ | $P[\lvert t\rvert > .10] \simeq .90$ | | $P[\lvert t\rvert > .74] \simeq .50$ | | $P[\lvert t\rvert > 2.03] \simeq .06$ | |
| *Group 5* | | | | | | |
| $n$ | 8 | | 11 | | 16 | |
| $\hat{\mu}$ | 1.39 | 2.61 | 1.48 | 3.52 | 2.55 | 3.45 |
| $\hat{\sigma}$ | .306 | .306 | .265 | .265 | .295 | .295 |
| $t$ | .35 | | .07 | | .17 | |
| $Pr[t]$ | $P[\lvert t\rvert > .35] \simeq .75$ | | $P[\lvert t\rvert > .07] \simeq .95$ | | $P[\lvert t\rvert > .17] \simeq .80$ | |
| *Group 6*** | | | | | | |
| $n$ | 11 | | 7 | | 15 | |
| $\hat{\mu}$ | 1.55 | 2.45 | 1.66 | 3.34 | 2.53 | 3.47 |
| $\hat{\sigma}$ | .151 | .151 | .374 | .374 | .142 | .142 |
| $t$ | .33 | | .43 | | .21 | |
| $Pr[t]$ | $P[\lvert t\rvert > .33] \simeq .75$ | | $P[\lvert t\rvert > .43] \simeq .65$ | | $P[\lvert t\rvert > .21] \simeq .80$ | |
| *Group 7* | | | | | | |
| $n$ | 4 | | 10 | | 6 | |
| $\hat{\mu}$ | 1.41 | 2.59 | 1.50 | 3.50 | 2.52 | 3.48 |
| $\hat{\sigma}$ | .086 | .086 | .161 | .161 | .249 | .249 |
| $t$ | 1.05 | | 0.00 | | .08 | |
| $Pr[t]$ | $P[\lvert t\rvert > 1.05] \simeq .35$ | | $P[\lvert t\rvert > 0.00] \simeq 1.00$ | | $P[\lvert t\rvert > .08] \simeq .90$ | |

* Matches resulting in single member coalitions omitted.

** Matches resulting in grand coalitions omitted.

about. Clearly, there is something wrong with an explanation in which individual irrationalities add up to group rationalities.

It seemed likely on the basis of unsystematic questioning, however, that the subjects were not really behaving irrationally in their individual choice. Instead of maximizing dollar amounts, they initially seemed to be maximizing expected values of the prizes. That is, they seemed to be discounting their anticipated share of the payoff to a coalition by the probability that the coalition would actually occur. Hence, the choice of a smaller amount over a larger might indicate simply a greater confidence that the coalition with the smaller individual payoff would actually form.

This concern for actual formation of a coalition provides a key to interpretation. The central interest of subjects lies not in the amounts won, but in the fact of winning. And if the fact of winning is the central concern, then the value of the prize is secondary. Indeed the subjective value of the prize is probably dependent on the chance one has of getting it. Thus a player proposes a given division of the payoffs because he feels that a coalition could form around this division and alters a proposed payoff structure in an attempt to increase the likelihood of forming a winning coalition. A player who wishes to win does not typically make proposals that he feels have little chance of acceptance.

In this view of subjects' goals, coalitions are seen as being *only* winning coalitions or losing coalitions with very little interest in the amount won. We assume that a player attempts to form that coalition which he perceives as having the highest probability of winning—i.e., he votes for the player he feels is most likely to vote for him. In this interpretation, then, utility maximization is maximization of the probability of winning.

In order to test for this possibility, subjects in groups 5, 6, and 7 were asked (after each set of three conversations) to estimate the probability that each of the others would vote for them, as well as probabilities for their own vote and for the formation of the several coalitions. To assure that these probabilities were not thoughtlessly estimated the questions were constructed so that the subject was forced to add them up to 1.00. The data from those questionnaires filled out just before the voting provides the basis for a test of the probability maximization interpretation.

Assume that the subject ordered his preferences so that, if $P_1$ and $P_2$ are the perceived probabilities of winning in each of two possible coalitions such that $0 < P_1 < P_2$, then the subject preferred $P_2$ to $P_1$ and $P_1$ to 0 and $P_2$ to 0. Note that this is an assumption of substantive rationality, that is, we have imposed a goal for the subjects to maximize toward and assumed that this goal is appropriate to the situation they are in. We know that his goal may not be wholly appropriate for at least two subjects, both of whom were fairly consistent losers. One subject in group five who, out of 10 matches, won only the first, third and ninth, became especially sulky after his eighth match. After a long dry spell, he had confidently expected to win because of a long term agreement made in his first match. But he lost and felt betrayed and announced he would play no more. He subsequently agreed to play the two further matches for which he was scheduled, but his heart was not in it. Although he actually won one of them, he behaved as if it were beneath his dignity to win and it is possible—though not certain—he preferred to lose. Another subject in group six, who, out of nine matches, won only the fourth and eighth, adopted a stance in bargaining that made him an almost certain loser. Midway in this series subjects invented a method of enforcing trust by exchanging objects of value (such as books, meal tickets, etc.) to be re-exchanged after a bargain to vote for each other was carried out. But the losing subject rejected all such bargains proffered on the ground, invented by himself, that they were not in harmony with the game. It seemed to these observers that his policy served the purpose only of providing himself beforehand with an excuse for anticipated failure. That is, he could say to himself, "I may be losing, but at least I am more ethical than these other fellows." So he too may actually have preferred to lose, for by guaranteeing himself a loss ahead of time he avoided thus the shame of losing when he was trying to win. But these are only two subjects out of 34 in the three groups and their possible preference for losing might have occurred in only six of their 19 matches. Using these two as contrasts, all other subjects appeared to the observers to want to win. We feel safe, therefore, in attributing to all subjects the preference order of $P_2$ to $P_1$, $P_1$ to 0, and $P_2$ to 0.

To determine whether or not subjects were maximizing the subjective probability of winning, let $\hat{p}$ $(ij)$ be the $i$th player's estimate of the probability that the $j$th player will vote for $i$, and let $\hat{p}$ $(ik)$ be the $i$th player's estimate of the probability that the $k$th player will vote for $i$. If subjects were maximizing probabilities, then their decision rule must have been the following:

If $\hat{p}(ij) > \hat{p}(ik)$, then $i$ votes for $j$;
If $\hat{p}(ik) > \hat{p}(ij)$, then $i$ votes for $k$;
If $\hat{p}(ij) = \hat{p}(ik)$, then $i$ votes for either $j$ or $k$:

On the basis of this analysis, we find that the subjects' voting behavior overwhelmingly supports this probability maximization model.[13] Almost 92% of the voting decisions studied (275 out of 300) can be explained by the probability maximization criterion. Considering that we were careful never to discuss their probability estimates with the subjects in order not to encourage them to rationalize their choices, 92% seems a remarkably high proportion of maximizations.

Yet not all of the 25 instances of failure to maximize can be consigned to the dark world of the irrational. For example, two subjects recognizing that they could more likely win by voting differently, nevertheless chose not to maximize in the current match in order to maximize in a future one. Thus in group 7, match 18, player 1 estimated his chances

from 3, $1.65 @ odds of .8 or $1.24
from 2, $1.45 @ odds of 1.0 or $1.45.

He voted for 3 and won, explaining his choice on the questionnaire thus: "I have 2, but am thinking of the future," (Since there were only six subjects in the group, each of whom was matched with each of the other four times, he knew for certain that he would meet 3, but not 2, in the one match remaining for him to play. In the conversation after the match, he indicated that this consideration had been the controlling one for him throughout the entire match and indeed he had used it as an argument to secure the adherence of 3.)

Thus of the 25 instances of non-maximization of the probability of winning, 11 appear to have involved an attempt to maximize dollars won. So it can be said that in 286 out of 300 cases, or about 95%, subjects behaved as if they were maximizing some utility of winning.

In the remaining 5% of the cases, however, subjects appear not to have maximized. Some failed to do so quite deliberately for they expressed (in remarks on the questionnaire) opinions that indicated other goals were greater for them than maximization of probability of winning. For some the sanctity of a promise was of greater value. Thus in group 6, match 16, player 3 (who lost) explained: "My word to player 1 is inviolable, although I don't think he considers it such. I'm probably throwing money away, but my vote is 1." (Player 3 was the one, previously mentioned, who almost always lost and refused to bargain in the accepted way because he—iconoclastically—believed this way to be out of harmony with the rules.) For others, it was more important than money to avoid hurting another. Thus in group 6, match 38, player 1, who by reason of a row between 2 and 3 (of which he had taken advantage) knew that both would vote for him, wrote somewhat shamedly on his questionnaire: "I really feel rotten about the whole thing. I don't want to screw anyone, so I flipped a coin to determine which player to vote for." For still others, gambling itself was of greater value than a sure prize. Thus in the last match in group 7, player 1, who was sure of $1.50 from 2 and less sure of $1.55 from 3, still chose 3, explaining: "I'll try for 3 'cause I'm a gambling man—can't lose that much—even though joining with 2 seems a surer victory."

Of the 14 instances of apparent non-maximization, most seem to be based on a sense of honor or the impulse to gamble. Were we using a procedural criterion of rationality, these would also be considered rational since they clearly preferred honor and some money to dishonor and more money. But since we are using a substantive criterion (higher probability of winning rather than less), we must count them as irrational. The remaining instances seem to be that kind of conventional irrationality best summarized by the subject (group 6, match 7, player 1) who wrote on his questionnaire: "total confusion, total insecurity, complete distrust." In these instances, subjects probably behaved irrationally because they were confused (or in some cases, stupid) and distressed by losing.

## V. EXPLANATION OF RESULTS

On the basis of the evidence just offered that from 92 to 95 per cent of the choosers tried to maximize the probability of winning and that the majority of others were apparently trying to maximize where some alternative other than winning stood higher in their order of preferences, it seems to us clearly the case that the probability maximization theory—here identical by our assumption to utility maximization theory—describes this surrogate political behavior very well. One can confidently expect, however, that proponents of other viewpoints will try to explain this fairly strong evidence away. Anticipating such reaction, therefore, we offer further evidence that, not only is probability maximization a good explanation, but also that it is a better explanation than the alternatives.

Those who assert that choosers do not employ a rational calculus but make up their preferences to fit some previously determined and affective choice are usually hard to answer because one has no way to get inside the choosers' psyches. So if it is said, for example, that player $i$ voted for $j$ because $i$ felt an intense homosexual attraction for $j$, it is usually not possible to prove or refute such an assertion. No evidence pro or con exists. And in the absence of evidence, the psychoanalytic and the probability maximization theories seem equally good (though equally unverifiable) explanations.

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[13] Details for the test of each case are available from the authors.

For this experiment, however, there is a way to get inside the psyches, at least a little bit, and thus to make a judgment on the adequacy of the alternative explanations. In this case, the choices made depend not only on the preference structure of the chooser but also on his estimate of the state of nature. One can, therefore, look at these estimates: Are they accurate or inaccurate? Do they apparently reflect an intent to make a careful estimate or do they reflect simply indifference?

We offer the following theory: If the subjects make accurate estimates of the actions of the others (the state of nature), then it can be supposed that, when they choose as if maximizing the probability of winning, they are in fact doing so. If, on the other hand, subjects make inaccurate estimates, then it can be supposed that the state of nature is irrelevant to their choice and that the apparent maximization is simply window dressing to cover up to themselves (i.e., reduce the cognitive dissonance occasioned by their Oedipal urges, etc.).

Suppose the subjects are found to make accurate estimates of the external world. Then the plausibility of the probability maximization theory is strengthened because it appears that subjects are making a careful estimate of their chances to win—the crucial feature of the probability maximation calculus. It appears also that in expending energy on careful estimates, subjects want to maximize. Suppose, however, subjects are inaccurate. Then the plausibility of the probability maximation theory is weakened for it appears to be based on a half-fictional calculation.

Conversely, for the antirational theories, inaccuracy about the external world renders calculation meaningless and suggests therefore that the subjects were not calculating to begin with—which is precisely what the notions of cognitive dissonance and psychoanalysis assert. Thus inaccuracy strengthens the plausibility of these theories. But accuracy weakens it, for if subjects expend enough energy to be accurate, it appears that subjects believe knowledge of the state of nature is important in their choice, in their calculation, indeed. Hence the probability estimates are more than window dressing and the plausibility of a theory that restricts them to this role is weakened.

In these experiments, subjects were on the average surprisingly accurate in their estimates of the state of nature (i.e., their judgments of how the other players were going to vote). The data is set forth in Table 2, where the estimates of a player about the behavior of others are compared to what the other players actually did. Since the subjects estimated a probability between 0 and 1 for receiving the votes of each of the others, while the voting itself was dichotomous, one cannot measure individual accuracy except in a gross way. But the accuracy of the entire group of judgments can be measured thus: For all those who estimated that another player would vote for them with a chance between 0 and .2, it should be the case, if the group is perfectly accurate, that ten per cent of those so estimated (i.e., the midpoint of the interval between 0 and .2) actually do so. For estimates in the interval .21 to .4, thirty per cent of those so estimated should actually do so, etc. Thus we have a definition of accuracy for the entire group. Inaccuracy, on the other hand, would occur if, for all those who estimate that another player would vote for them within a given interval of probability (say, again, 0 to .2), it is actually the case that fifty per cent of the others actually do so. Thus inaccuracy is discovered by the fact that voting by the others is random with respect to the subjects' estimates.[14]

The results stated in Table 2 are depicted in Figure 1, where the per cent voting for the estimator (on the vertical axis) is plotted against the estimates in intervals of .2 (on the horizontal axis). The straight lines in the Figure represent perfect accuracy and random behavior or inaccuracy, while the curved line indicates the actual degree of the subjects' accuracy about the other players' behavior.

It is apparent from Figure 1, that subjects were on the average quite accurate with respect to the behavior of the other players. The line connecting the per cent voting for in each of the five categories is close to and has about the same slope throughout as the line indicating perfect accuracy. Moreover, it has been demonstrated elsewhere that these discrepancies from perfect accuracy can be attributed to sampling error.[15] On the basis of this evidence, therefore, it seems reasonable to reject anti-rationalist theories conclusively as explanations of this surrogate political behavior.

It is not so easy to reject learning theory. For the events of these experiments, an explanation in terms of learning theory would empha-

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[14] We wish to thank Gerald Kramer for help in developing this test.

[15] William James Zavoina, *The Rational Calculus of Coalition Formation,* unpublished Ph.D. thesis. University of Rochester (1969).

TABLE 2. ACCURACY OF SUBJECTS' ESTIMATES OF OTHER PLAYERS' VOTES

| Probability of Receiving Other's Vote | Subjects' Estimates of Other's Votes | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | 0.0–.20 | | .21–.40 | | .41–.60 | | .61–.80 | | .81–1.00 | | Total |
| Others' Actual Vote | For | Against | For | Against | For | Against | For | Against | For | Against | |
| Group 5 less victims of conspiracy* | 1 | 31 | 10 | 11 | 30 | 31 | 40 | 33 | 38 | 15 | 240 |
| | | (−2) | | (−1) | | (−4) | | (−8) | | (−3) | (−18) |
| Group 6 less victims of conspiracy* | 4 | 32 | 9 | 19 | 34 | 45 | 29 | 20 | 43 | 5 | 240 |
| | (−1) | (−1) | | | (−1) | | | | | (−1) | (−4) |
| Group 7 | 4 | 4 | 3 | 16 | 5 | 16 | 17 | 16 | 31 | 8 | 120 |
| Total Votes | 8 | 64 | 22 | 45 | 68 | 88 | 86 | 61 | 112 | 24 | 583 |
| Percent Received | 11% | | 33% | | 44% | | 60% | | 83% | | |

* In eleven matches, two of the players formed a conspiracy against the third thus: *i* and *j* agreed on a satisfactory imputation and on a plan of conversations of *i* with *k* and *j* with *k*. This plan typically provided that both would falsely report to *k* an (*i*, *j*) agreement highly advantageous to *j* and that *i* would then seek a better agreement with *k* though still not as good for *i* as the real (*i*, *j*) agreement, while *j* would seek an agreement with *k* (also not as good for *j* as the real (*i*, *j*) agreement) on the ground that he (*k*) distrusted *i*'s extravagant offer. Functionally, this served to guarantee to both *i* and *j* that they were better off with each other and that they could trust each other. Of course, it also meant that *k*'s judgment of the external world had no relevance to what that world was really like. Since his judgment concerns a fictional world created for him by the conspirators, we have omitted *k*'s judgments from this table.

size that subjects determined their partners, not by a calculus but by more or less random experimentation which, if successful, would be repeated and, if not, not. And indeed something of this appears to have happened. One can reject learning theory, therefore, not because it fails to explain behavior, but simply because it does not explain it as well as the utility maximization theory.

In the 100 matches studied here, there were, of course, 300 pairs of subjects. Of these 160 pairs were inexperienced with each other in the sense that they had not met previously in a match—and in most cases had never met consciously at all. There were, however, 140 pairs in which subjects were experienced with each other in the sense that the members of the pair had played with each other in a previous match, in some cases in two or even three previous matches. While learning theory could hardly say much about behavior of subjects in the 160 inexperienced pairs, it can say something about the behavior in the 140 experienced ones.

In general, learning theory suggests that, for these 140 pairs, if previous experience had led to success (i.e. voting for or against and winning), then the members of these pairs would repeat the previous choice. If previous experience had led to failure (i.e. voting for or against and losing), then the prediction is not so clear. However, if on previous experience in a pair, a player voted for and lost, it would seem to be a good indication to vote against on the second match, if only because he knows that voting for is not successful. On the other hand, if a player voted against another and lost on the first meeting, there is almost no guide to action. He knows only that voting against the other did not work, but he is quite inexperienced about the results of voting for. In such case, there is no really good prediction inasmuch as it is reasonable either to vote for as an experiment or to vote against on the basis of experience.

The data to validate these predictions is set forth in Table 3, where the individual experience on the just-previous match—earlier matches, when they occurred, are ignored—is compared with the action taken by the individuals in the current match.

As is apparent from Table 3, the tests of the predicitions from learning theory are in part beyond reasonable chance. But still the explanation

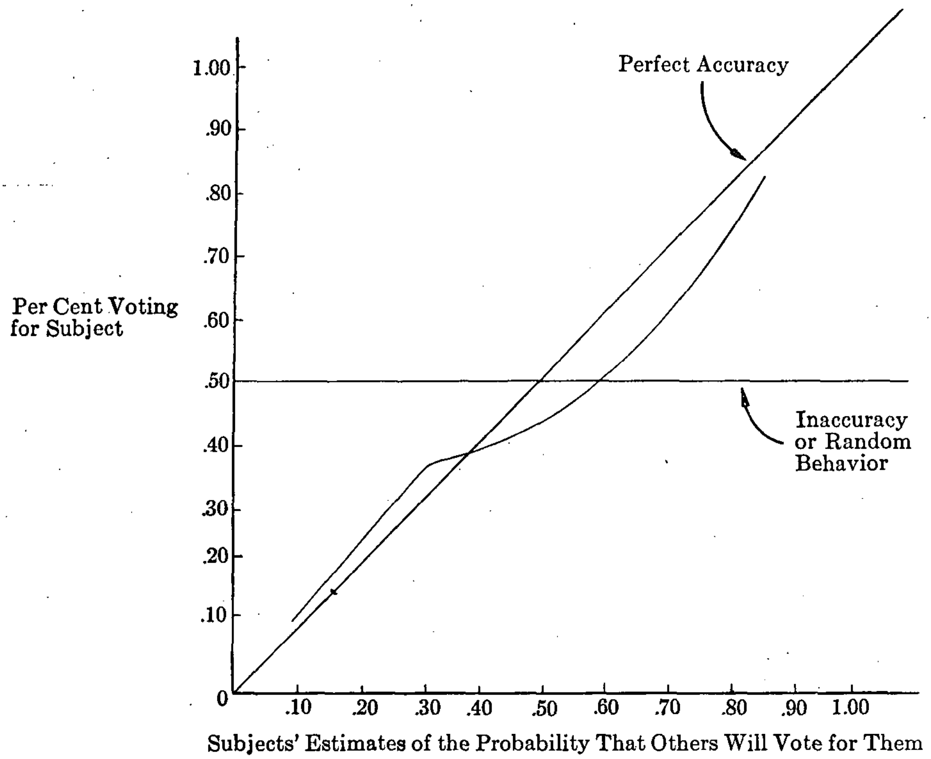

Figure 1. Accuracy of Subjects' Estimates of the State of the External World

is not as good as the explanation from utility theory. In the first place, learning theory says nothing about the majority of the cases for the subjects had to act in an environment that denied them a previous experience. (Some will say this is not a fair test of a theory; but then the world is full of new conditions and the rat has to go through the maze a first time.) Even when a previous experience was provided, however, the explanation from learning theory is weak. Only 43 per cent of the relevant behavior is explained (i.e., that recorded in the first three columns of row one of Table 3), while in the probability maximization theory from 92 to 95 per cent of the relevant behavior is explained. Furthermore, only the behavior summarized in column 1 of Table 3 is explained well because the favorable results of the test in columns 2 and 3 may easily have occurred by chance. On the other hand, the possibility that chance may have produced the result in the test of probability maximization is almost zero. This is, the probability that one might obtain by chance 275 "yes" out of 300 tries is less than .000001. This value is considerably smaller than that associated with the best prediction under learning theory. Thus, comparing the two viewpoints, learning theory only weakly explains behavior in the limited range of behavior to which it applies, while the theory of probability maximization explains behavior most of the time. Certainly, for this surrogate political behavior, probability maximization appears to be the more plausible theory.

## VI. CONCLUSION

In the introduction we observed that each of the viewpoints toward rational behavior was supported by good evidence in the area of behavior to which the viewpoints were originally applied. Furthermore, we pointed out that, owing to the neglect by political theorists, we had no idea of which area of behavior was the one where political behavior typically belonged and hence we had no idea of what viewpoint toward rationality was appropriate for political science. Finally, we indicated that, whether we wanted to or not, our ignorance on this point had to be repaired if we wished to do sophisticated political studies.

This essay is an effort to repair this ignorance. Unfortunately it deals with surrogate politicians in a surrogate political setting. Since this is probably the best we can get, however, the conclusion that the theory of probability maximization is a more plausible explanation than its alternatives for the behavior studied suggests

TABLE 3. VOTING IN EXPERIENCED PAIRS

| | | Experience of Subject in Just Previous Match Involving Other Player in Pair | | | |
|---|---|---|---|---|---|
| | | Voted For | | Voted Against | |
| | | Won | Lost | Won | Lost |
| Action of Subject in Current Match | Votes For | 63 | 22 | 36 | 27 |
| | Votes Against | 31 | 26 | 50 | 25 |
| | | 94 | 48 | 86 | 52 |

| Theory | Truth Value | Binomial Probability |
|---|---|---|
| cell (1, 1) > cell (2, 1) | True | [*]$Pr$[63 For] < .00063 |
| cell (1, 2) < cell (2, 2) | True | $Pr$[22 For] < .33 |
| cell (1, 3) < cell (2, 3) | True | $Pr$[36 For] < .08 |
| cell (1, 4) ≃ cell (2, 4) | Approximately True[#] | |

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[*] That is, assuming independent choices with a .5 probability of choosing For, the probability of obtaining by chance 63 or better Fors out of 94 tries is less than .00063.

[#] Since there is no standard of randomness here, a probability of deviation from it cannot be calculated.

that in the interpretation of real political phenomena it is safest to assume that politicians are calculating maximizers.

Doubtless, however, some will resist the analogy from the game saying that the behavior studied was "really" economic rather than political since it involves a monetary reward. But at least since Marx most students of politics have been willing to admit that all of politics involves monetary rewards. And certainly the calculating behavior in this game involves something more than the economic process of maximizing a monetary reward inasmuch as the subjects had to maximize in the face of a highly political kind of uncertainty about how others would vote.

Doubtless, also, some will resist the analogy saying that the behavior studied was simple compared to the complex world of real politics. But it is hard to be sure about degrees of complexity. Surely our subjects often faced a world fully as uncertain as the world of politicians. And while they had but two alternatives to choose from, it is also true of the political world that politicians constantly seek to narrow their range of choice, not only in personal decision, but also in our institutions. Thus, we vote on two candidates, we vote yea and nay on motions, we are offered two parties (or, failing that, government and opposition), etc., etc. In short, in the world outside the laboratory, we seek constantly to make the problem of decision no more complex than the decisions facing our subjects. So far as we succeed, there is not much difference in complexity between the two worlds.

Nevertheless, still others will resist the analogy saying that, even if politicians succeed in simplifying their decision problems, still the laboratory scene and the people in it lack all that lush growth of ideology and organization found in the real political world. This we of course admit; but it still is true that our subjects, while simply students, were drawn from the class of people who grow up to be politicians and that the laboratory scene did have those features of coalition-formation winning and losing, manipulation of others by both rhetoric and contrivance, etc.—all of which are fundamental to politics and are the building blocks of ideology and organization.

It is possible that one kind of real political situation exists for which the game analogy is quite inappropriate and this is the situation where participants are not conscious of having a choice. One can hardly compare alternatives where there are none to compare. And it may be that much of the time people have no choice, though one strong theological and philosophical tradition insists that choice is always present. But where choice is possible, the message in the evidence from this experiment, though indirect, is crystal clear: utility maximization is the theory that fits political behavior best.

# BARRINGTON MOORE AND THE DIALECTICS OF REVOLUTION: AN ESSAY REVIEW

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"When I use a word," Humpty Dumpty said, in rather a scornful tone, "it means just what I choose it to mean—neither more nor less."

"The question is," said Alice, "whether you can make words mean so many different things."

"The question is," said Humpty Dumpty, "which is to be the master—that's all."

*Through the Looking Glass*

## I. INTRODUCTION[1]

It is not hard to find reasons why Barrington Moore's *Social Origins of Democracy and Dictatorship* has had such widespread influence.[2] Its approach, that of comparative, historical sociology, seeks clues to the present in the past, and Moore demonstrates mastery of a wide range of historical materials. Yet I feel that the book is ultimately unsatisfactory, for it is marred by a lack of respect for its own sources of information and by contradictions and *non-sequiturs* at critical points in the argument.

In this critique I shall first examine the general thesis of the book and then turn to the case studies which Moore uses to support his arguments. I shall also attempt to account for the sharp contrasts in the quality of scholarship which characterize the study, and will attribute them to Moore's preconceived ideological assumptions about the nature of the good society.

## II. THE OVERT ANALYSIS

The central argument of the volume seems to be about as follows:

Historically, societies have industrialized in one of three ways. Each of these has involved violence and the exploitation or oppression of a large bulk of the population by a "ruthless" ruling class. (24, 28–29, 287, 297, 386, 410, 506.)[3]

The first route taken by England—and with variations by France and the United States—may be called the bourgeois-capitalist-democratic road. This route required a violent revolution designed to destroy the power of at least a portion of the traditional elite. It also involved the "ruthless" exploitation of both the peasantry and the working class in the early stages of industrial development.

The second road was taken by Germany and Japan. Here the bourgeoisie, too weak to assert itself, joined with a quasi-feudal landowning class in an attempt to modernize society within its traditional social framework. Industrialization was not achieved without exploitation, but revolutionary upheaval was avoided. However, the ultimate outcome was Fascism, and in Germany at least, genocide.

The third route, that of Communism, has occurred in those societies in which the ruling classes could not successfully industrialize. Communist regimes generally come to power on the backs of peasant revolutions, and end with the elimination of the peasantry and the development of a repressive system comparable to although different from that in capitalist countries.

One of these three roads, Moore argues, has been more or less inevitable, and together they exhaust the possibilities of social change. Today the bourgeois capitalist route to modernization seems obsolete, the Fascist road too horrible to contemplate, and even the traditional Communist road (i.e., the Soviet road) historically dated, although China may avoid Russia's mistakes.

Indian political leaders have attempted to avoid these choices and to modernize democratically. Moore thinks they have failed, and he hints fairly strongly that an authoritarian regime will have to emerge if India is to solve her problems.[4]

The evidence Moore brings to his argument is to be found in his case studies. Thus, his choice of cases becomes crucial. Moore could not deal with every country in the world, but his general theory of social change, so far as I can determine, is not at all applicable to black Africa or to the Middle East, and, thus, to much of the "third world." In addition its applicability to countries like Sweden or Norway seems problematical.[5] Moore specifically excludes the Scandinavian countries from his discussion, arguing that, since they are small countries which "depend economically and politically on big and powerful ones . . . the decisive causes of their politics lie outside their own boundaries." (xiii) Moore offers no support for this argument, yet deals in great detail with at least one country—India—which *was* influenced in very decisive ways by an outside power. One suspects, then, that other reasons lay behind his choices.

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[1] Professors R. Jackson Wilson, Daniel Aaron, Cecelia Kenyon, Murray Kitely, Guenter Lewy and Malcolm Smith all read an earlier version of this essay and offered criticisms. Professors Wilson and Aaron were especially helpful. Naturally, the errors which remain are mine.

[2] (Boston, 1966).

[3] As with many other of his statements, Moore occasionally hedges (508, for example), but this is the overall thrust of his analysis.

[4] The general argument is summarized on pp. xv–xvi, 413–414. On the obsolescence of bourgeois capitalism and Russian Communism, see pp. 507–508, 483. On Moore's hopes for China, see p. 230. I will add additional references in the sections on specific countries.

Of more significance at this point are Moore's neo-Marxist assumptions as to the underlying pattern of social causation. He argues (1) that the mode of production in a given society determines ideology of the social classes of which it is composed and its political structure, and (2) that all ruling classes have as their primary goal the fullest possible exploitation of those whom they dominate. Ecological factors are unimportant and cultural variables are epiphenomenal since they either reflect the economic system or are mechanisms used by the ruling classes to further their own interests. Thus, Northern commitment to an anti-slavery position in the American Civil War is to be explained by the emergence of an urban capitalist civilization; and Hinduism and the caste system, symptoms of India's historical backwardness, both survived only because they served the interests of the ruling strata. (123, 136, 335–36.) Nor are Japanese cultural patterns relevant to an understanding of the Meiji Restoration, which was based entirely on the self-interest of a particular segment of the ruling class, just as the support of social legislation by the English bourgeoisie in the mid-nineteenth century was merely a sop to the lower orders, because everyone knew that the bourgeoisie now dominated English politics. (240, 245, 441.)

The mode of production and class exploitation are key to Moore's overall historical analysis. Past revolutions have been inevitable, because the ruling classes were unable to transcend their own ideologies and unwilling to accept limitations upon their opportunities for exploitation.

Moore essentially claims that his frame of reference enables him to systematize the study of comparative history more effectively than alternate models, and that he was forced to his conclusions by the facts themselves. (356, 505 and the Preface.) However, Moore also levels a sharp theoretical attack upon models that give more weight to cultural variables, specifically the work of Max Weber, who emphasized the uniqueness of European history, and developed what is perhaps the most powerful modern theoretical alternative to a Marxist or neo-Marxist analysis.

An overall evaluation of the value of Moore's categories must await an examination of their utility in dealing with the various countries with which he is concerned, but the following reflections are not out of order at this point.

First, Moore's two central propositions have no necessary connection. The values of a given social class may be determined by its economic position, but this does not prove that these values necessarily involve the maximization of its opportunities for "exploitation."

Further, the propositions are not equal in explanatory value. The discovery of economic correlates for the *Weltanschauung* of a particular class may be offered as evidence that these factors were causal, but to assert that a given *Weltanschauung* is supported because it is self-serving explains little or nothing. Even if the ruling classes in India were only interested in exploitation, this would not tell us why they chose Hinduism and the caste system as their instruments. Moore's volume, as we shall see, is full of question-begging explanations of this type.

Sometimes, as a last resort, Moore does give an independent role to cultural and ecological variables, even though he would seem to weaken his thesis in the process. For example, what is one to do with an analysis which continually downplays the role of ideas and then lists as one reason for the mild treatment of the Chartists by the English ruling classes the fact that:

> Russell was a doctrinaire Whig devoted to the ideal of liberty and anxious to avoid encroaching on the free discussion of political issues. (34.)

There are many other instances of the *ad hoc* introduction of other variables. After emphasizing economic factors in the behavior of the English bourgeoisie, Moore suddenly brings in England's reliance upon a Navy (stemming, one assumes, from the fact that the country is an island) as among those factors which explain the lack of repressiveness of English society. (32, 444.) It is almost the only time that this variable is mentioned with regard to England, and Moore doesn't even consider why Japan's island position did not play a similar role. Or, to take another example, he argues that Japan's imperialism was the result of economic contradictions which led to Fascism. English imperialism, on the other hand, was the result of:

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[5] For that matter the analysis seems inapplicable to those countries in Southeast Asia, e.g., Thailand, which, for all practical purposes, lack a landowning gentry. On Thailand see: David A. Wilson, *Politics in Thailand* (Ithaca, 1962), p. 52; James Mosel, "Thai Administrative Behavior" in W. J. Siffin (ed.) *Toward the Comparative Study of Public Administration* (Bloomington, 1959), pp. 283–288; and D. Insor, *Thailand* (New York, 1963), pp. 44–45.

> . . . a combination of adventure, reasons of state, commerce, and plunder: motives and causes that were really indistinguishable during that burst of energy released all over Europe by the decay of the traditional Christian medieval civilization and the rise of a new and much more secular one. (342.)

Moore's habit of blending quasi-economic determinism with other whimsically introduced factors, including cultural variables, makes it hard to systematize his position, and raises serious questions about his interpretation of the societies which he uses to illustrate his general theory. Indeed, since he offers no explicit criteria one gets the impression that decisions as to what weight he should accord cultural factors were based less on a concern with placing his material in a theoretical perspective, than upon a desire to convince his readers of the validity of assumptions brought to his study.

Moore's discussion of exploitation suffers from similar limitations. He considers the concept objective and measurable. In fact, he lists the criteria which determine whether a given class relationship is exploitive. But, although in theory he allows for gradations in exploitation, the English, French, German, Japanese, Indian and Chinese aristocracies all seem to have shared one aim, that is, to squeeze as much out of their peasants as possible.

One wonders whether a concept which is not used to differentiate among societies, has any empirical utility. The confusion is increased by Moore's tendency to hedge. Peasants, he argues, revolt because of objective exploitation. If they revolt even while their objective situation is improving, it is only because they realize that the "lord's exactions increased and his contribution . . . declined." (473.) If they don't, their failure to revolt is explained by inertia. As for those peasants who resist the revolution, Moore redefines them as being part of the exploiting classes because of shared ideas. Thus they present no analytic problem. Moore says of the French Revolution that:

> To be sure, there were members of roughly the same social strata fighting on opposite sides. But they were fighting for opposed social objectives. . . . Victory for one side or the other meant the victory or defeat of class privileges. On these grounds alone it seems impossible to deny that the Terror was an instrument of class warfare. . . . (518.)

Furthermore the evidence which he offers to prove that a given ruling class has been entirely exploitive and almost entirely parasitic is not always very compelling. For example, he argues that the Chinese gentry fulfilled hardly any useful function in Chinese society. (203–05.)[6] Most contemporary scholars, on the other hand, would contend that China attained relatively high levels of general prosperity, social order and cultural and commercial development during the height of the Ch'ing dynasty in the seventeenth and early eighteenth centuries. Certainly the political order must have played some role.[7] But, Moore argues that the increase in population which occurred at that time and which most observers might take as a sign of effective social organization and rising living standards, did not stem from these factors. Rather it was the gentry who increased the birthrate of the peasants in order to drive up the price of land and to impoverish the masses. (168.) On the face of it the argument is less than convincing. During the early part of the period, at least, living standards continued to rise despite the increase in population, partially because the regime sponsored the opening of new areas for cultivation and introduced new crops.[8] Ping-ti-

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[6] Moore does mention the improvement of irrigation, but downplays its importance. Indeed the major purpose of improving irrigation was to increase the possibilities of exploitation. (169.) He also argues that its role in the maintenance of order was unimportant, given the place of the clans in administering local justice. Besides, when large scale banditry did develop it was largely the consequence of exploitation by the ruling class. What evidence, one wonders, is relevant to the question of measuring exploitation? Moore, in effect, has it both ways. If a ruling class does literally nothing, it is clearly parasitic, but whatever functions it performs are performed for bad reasons and it is still parasitic.

[7] See Ping-ti Ho, *Studies on the Population of China, 1368–1953* (Cambridge, 1959), pp. 268–270. Moore cites Ho several times but appears to deny that living standards were rising for the population as a whole. (*Origins,* 179.) Interestingly enough, however, he does argue, at least indirectly, that China was better off than India in this regard, and Japan was better off than China (*Origins,* 330–333 and *passim.*).

[8] Ho, *op. cit.,* 269. Edwin O. Reischauer and John K. Fairbank, *East Asia: The Great Tradition* (Boston, 1958), I, p. 393.

Ho, whom Moore relies on for population data, argues that the Chinese were hoist by their own petard. Having created conditions of general prosperity, the regime was finally faced by over-population in relation to resources, under circumstances in which it had reached the limits of its capacity to raise levels of productivity within the framework of the existing system.

Whether or not the Chinese gentry did at one time serve some function in Chinese society has little to do with the general conclusions which Moore reaches about the necessity for a modernizing revolution in China. The difference between Moore's view and one which emphasizes the irony of Chinese development is a difference between a view of history as involving genuine tragedy, and one which sees it as a conflict between heroes and villains. Despite Moore's explicit disclaimers, it is quite clear that the latter is his perspective.

Finally, Moore's stake in proving that all ruling classes are completely exploitive sometimes leads to contradictory appraisals. Thus, he usually describes Feudalism as "gangsterism." But, in talking of modernizing elites, he concedes that feudal arrangements had at least protected the peasants against grosser forms of exploitation.[9]

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Moore offers several reasons for rejecting approaches to the study of society which give significant weight to cultural factors. He implies, first, that such explanations tend to emphasize continuity and downplay change, and thus have a conservative bias. (485–86.) There seems little intrinsic reason why this should be so, and Moore offers none. Max Weber, whom Moore spends a good deal of time criticizing, uses cultural variables, in part, to explain the great European revolutions which began in the seventeenth century, and Robert Heilbronner has come to conclusions about the third world at least as radical as those of Moore, by emphasizing the role of cultural impediments to peaceful change.[10] But, even if the charge were true, it would not constitute an adequate scholarly reason for rejecting explanations of this type.

Nevertheless, his attack on the use of "cultural-continuity" explanations is worth examining in detail because it undermines his own approach, although he may not realize this. "The assumption . . . that cultural and social continuity do not require explanation," notes Moore:

> obliterates the fact that both have to be recreated anew in each generation, often with great pain and suffering. To maintain and transmit a value system, human beings are punched . . . cajoled, bribed, made into heroes, encouraged to read newspapers, stood up against a wall and shot. . . . To speak of cultural inertia is to overlook the concrete interest and privileges that are served by indoctrination . . . (486.)

Earlier he had argued:

> Such explanations are logically the same as the famous explanation of the effects of opium as being due to its "dormitive" properties. They beg the fundamental question: why did this particular outlook prevail when and where it did. (240.)

Moore is unfair. Those who argue that the cultural inheritance of a society is a significant part of any explanation of its dynamics do so on the basis of some fairly sophisticated propositions concerning the transmission of cultural values. And Moore himself, despite his strictures, is sometimes forced to recognize the "stickiness" of cultural patterns. In the same chapter from which the above quotation is taken, he explains the revolt of at least some samurai against the new Japanese regime as a function of the difficulties of shedding romantic "feudal notions." (252.)

More importantly, as Moore's own "question-begging" explanations indicate, economic self-interest does not exist in a vacuum. If we must take account of the concrete interests that are served by adherence to certain cultural norms, the reverse is also true; men define their interests in terms of the values which they deem important.

Further, Moore himself tends to emphasize continuity, albeit in a rather different way from those whom he criticizes. For example, in explaining the acceptance by the Chinese peasant of a regime which was fundamentally exploitive, Moore argues:

> . . . it seems more realistic to assume that large masses of people, and especially peasants, simply accept the social system under which they live without concern about any balance of benefits and pains . . . unless and until something happens to threaten and destroys their daily routine. Hence, it is quite possible for them to accept a society of whose working they are no more than victims. (204.)

Is this explanation of continuity any more satisfactory than a "cultural" explanation? Indeed it is less so, for it does not suggest any mechanism by which ideas about these institutions are transmitted. Unlike most sociologists concerned with the transmission of culture, Moore puts considerable emphasis on the violence done the individual by the socialization process. Somehow, he seems to feel that this weakens an argument which takes cultural continuity for granted. But why? If the process of socialization were as brutalizing as he makes it out to be, it might strengthen the hands of those who urge that continuity is natural, and change that must be explained. Having given up so much in the process of becoming part of any society, adult members of that society would resist proposed changes in the value structure with considerable ferocity. In short, Moore's argment proves nothing.

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[9] Compare, for example, *Origins,* 10, 214, 241 with *Origins,* 12, 194.

[10] In "Counterrevolutionary America," *Commentary* (April, 1967), pp. 3–10.

In criticizing Max Weber Moore wishes to undermine an interpretation of European developments which stresses the role played by Christianity and more specifically English Calvinism. His basic argument is that one can find the characteristics associated with the Protestant ethic in all of the societies under study, and that therefore the emergence of Calvinism cannot be used as an explanation for English economic and political developments. But, as Reinhard Bendix points out, Weber never argued that the Protestant ethic was the only reason that capitalism emerged first in Europe. Rather, his analysis deals with Protestantism as one variable among others.[11] Secondly, in proving the existence of a "Protestant ethic" in India, or China, or Japan, Moore relies heavily upon travelers' reports. These, of course, are of very limited value, and Weber never denied that at least some of the values associated with the "Protestant" ethic might be found to a limited degree among segments of any population.

Further, Moore has a very limited understanding of the attitudes which Weber believed characterize modern capitalism. In dealing with France, for example, he quotes Forster's study of the nobility of Toulouse to prove that many of the provincial nobility were characterized by "thrift, discipline, and strict management," as well as a powerful desire to increase their wealth. (51.) As Moore sees it, this shows that their attitudes were the same as those described by Weber as constituting the Protestant ethic. The only difference between the French nobility and the English gentry was that the former, instead of rationalizing their estates, "used the prevailing social and political framework to "squeeze more grain out of the peasants and sell it." (53.)

Whether or not this description of the French nobility is accurate, it does not support Moore's argument. Weber never argued that the desire to increase one's wealth or even the organized effort to do so was a peculiarly capitalist phenomenon. What differentiates the capitalist orientation from pre-capitalist orientations is not only its greater rationality in terms of organization, but also its emphasis upon mastery of the environment. Profit is the unintended consequence (and in some ways unwanted consequence) of this Calvinist drive toward mastery, toward changing the external world through labor. Thus profits based primarily on colonial or fiscal exploitation, on the basis of force guaranteed by political authority, i.e., continuous earnings through compulsory payments, or various methods of tax-farming, are considered by Weber to be part of pre-capitalist economic structures.[12]

Indeed a good many scholars have pointed to just the characteristics Moore describes, in contrasting both the French aristocracy and the bourgeoisie with their English counterparts before and after the Revolution. Their point is that French attitudes remained pre-capitalist in so far as they were not innovative.[13] Forster himself is careful to point out that the limited sample of provincial nobles he studied had made very few efforts to follow the English example of exploring new techniques for raising the general level of productivity.[14]

Moore's attack on Weber is less than successful. It remains to be seen whether or not the application of his own frame of reference to the countries studied is more adequate than one derived from a Weberian model.

## III. THE WESTERN CASE STUDIES

The assumptions which guide Barrington Moore's analysis reveal themselves only gradually in his historical case studies, and it is to these we must now turn. My knowledge of the material varies widely with the countries involved, and my analysis will not be directed primarily at "disproving" his argument on the basis of the evidence, but rather on pointing up some of the contradictions in his analysis.

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[11] See Reinhard Bendix, *Max Weber: An Intellectual Portrait* (Garden City, 1962).

[12] Bendix, *op. cit.*, 53.

[13] See, for example: Bert F. Hoselitz, "Entrepreneurship and Capital Formation in France and Britain Since 1700," in National Bureau of Economic Research, *Capital Formation and Economic Growth*, p. 311; David Landes, "French Entrepreneurship and Industrial Growth in the Nineteenth Century," in Barry E. Supple (ed.), *The Experience of Economic Growth* (New York, 1963), pp. 340–353; and George V. Taylor, "Non-capitalist Wealth and the Origins of the French Revolution," *American Historical Review*, 72 (January, 1967), pp. 469–496.

[14] Robert Forster, "The Provincial Noble: A Reappraisal," *American Historical Review*, 68 (April, 1963), p. 687.

Moore's Western case studies are confined to England, France, and the United States, with briefer scattered references to both Germany and Russia. Although I will stress those aspects of Moore's work with which I disagree, I should note that the differences are only in emphasis, for much of what he says is part of a general scholarly consensus. However, as in most matters of social analysis, differences of emphasis often result in radically different conclusions.

England, to Moore, is a paradigm case of industrialization, and his chapter is designed primarily to prove three propositions. First, he attacks the notion that the English have some peculiar knack for settling social disputes peacefully. (4.) The relative peacefulness of English society in the eighteenth and nineteenth centuries was preceded *necessarily* by an outburst of violence in the seventeenth century. Without this violent revolution produced by intemperate and violent men, the later development of peaceful parliamentarianism would have been inconceivable. (20, 39.)

Second, the English civil war was essentially a contest between two societies based on differing economic systems, between the traditional aristocracy, and the commercially oriented gentry supported by urban commercial interests. (14–20.)

Third, the industrial revolution in England was accompanied by "massive violence exercised by the upper classes against the lower," (29.) which was not different in kind from that exercised by other "ruthless" elites during the process of industrialization.

Competent historians, of course, do not subscribe to a non-violent myth of English history any more than does Moore. The fact remains, however, that even the English Civil War was relatively mild by continental standards, something which Moore himself recognizes and attempts to explain. The Civil War aside, it is still the relative peacefulness of British evolution since the eighteenth century which poses the more interesting problem.

For his economic interpretation of the Civil War, Moore relies heavily upon Tawney's "rise of the gentry" thesis, a position which commands far less than universal support among English scholars today, including some of those he cites.[15] Moore relies, too, upon an analysis of the geographic distribution of the opposing forces in the Long Parliament, noting, as have all previous commentators, that the more "commercial" sections of the realm generally supported the Parliament, while the more backward generally supported the monarchy. (510–514.) The problem lies in the fact that even in the areas supporting the Parliament, there was strong latent support for the king, and, further, these were the same areas in which the dissenting sects were strongest.[16] Moore's case for a basically economic interpretation of the revolution, one which de-emphasizes religious and other factors, is not very strong.

Indeed Moore seems to be in a quandary, for he admits in the end that the Civil War did not result in the displacement of one class by another. Its major contribution to bourgeois democracy seems to have been the killing of an English king—a killing which served as a stern reminder to later monarchs. (18, 17.)

The killing of the king, of course, was a political rather than an economic act. It is not clear why it should have been so traumatic. As Plumb and others have pointed out, this was far from the first king the English had killed. Down to the seventeenth century the English had something of a reputation as regicides.[17] What caused the trauma? If, as Moore suggests, the Civil War did not produce a fundamental alteration in the class structure of the society, it is at least possible that the changing political culture of

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[15] He cites Lawrence Stone as more or less supporting Tawney's thesis. (*Origins*, 15). Stone, however, specifically rejects his own previously held belief in the key role played by such factors. Indeed, he calls for the replacement of economic interpretations of the Civil War by broader sociological interpretations, and argues that other factors, including religious factors, played a key role in the conflict. See Lawrence Stone, *The Crisis of the Aristocracy* (New York, 1967; Abridged edition), pp. 4, 7–9, 345–354.

[16] See, for example, Ivan Roots, *The Great Rebellion* (London, 1966), pp. 62–68; Stuart E. Prall (ed.), *The Puritan Revolution* (New York, 1968), ix–xxii, and Charles Wilson, *England's Apprenticeship* (New York, 1965), pp. 108–140.

[17] J. H. Plumb, *The Growth of Political Stability in England* (London, 1967), p. 19. In the 200 years before Henry VII established the Tudor dynasty, England had been ruled by twelve monarchs. Only three reigned until death overtook them. Of the remaining nine, seven were deposed, five of whom lost their lives by violence, and two went insane.

English society played some role, and that religious factors played at least as important a role in these changes as did economic.

No one seriously doubts the importance of economic factors in explaining changes in political and other values in England and elsewhere, but the evidence indicates that religious and constitutional issues were at least as significant, and were not merely dependent variables. Wide agreement had been reached in the Long Parliament on purely economic issues; it was the political and religious question, as well as Charles' personality, which ultimately produced the conflict.[18] It is hard to believe that the English Civil War could have occurred without the political aggressiveness of the dissenting sects, and there is every reason to believe that another Monarch might have been able to escape with his head.[19]

There is little evidence that enclosures or the commercialization of agriculture were greatly accelerated by the Civil War itself, and, in any event, the dissenting sects were rather more favorably inclined to enclosure than other religious groups.[20] I should note that while Moore sometimes sees the Civil War as opening the floodgates to enclosure, he offers no evidence for this assertion, and at other times is quite willing to admit that the process was a gradual one until the late eighteenth century, to which we may now turn. (19, 25.)

Moore's analysis of English industrialization represents what must be called a "post-revisionist" position, in that he sharply criticizes those scholars who have tried to alter the traditional picture of the consequences both of enclosure and of industrialization. Before we analyze his attack in somewhat more detail, let us consider briefly the revisionist account of the changes which took place in late eighteenth and early nineteenth century England.

First of all, such scholars as Mingay, Jones and Thompson deny that the wave of enclosures which swept over England in the late eighteenth century served to break the back of the small peasant proprietor and to convert him into a rural or urban proletariat. They argue that while the concentration of landownership by about 1830 was greater than in the seventeenth century, it was not much greater, and that the increase had been a very gradual one.[21] In fact, while the enclosures of the late eighteenth century took land from some peasants, others received land. The evidence seems to indicate that the number of small owner occupiers actually increased for a short time.[22] The proportion of agricultural workers to owners was about 1¾ to 1 at the end of the seventeenth century; 140 years later it had risen to only about 2¾ to 1.[23] The real source of the gradual decline in small holders was not enclosure, but the relative disadvantages of small-scale agriculture in an era of commercial farming.[24]

Revisionist scholars argue further that enclosures did not depopulate the countryside. Instead the rural population actually *increased* during the period. In many cases the largest migrations took place from unenclosed areas, or areas which had been enclosed much earlier. The creation of an urban and even a rural proletariat was far more the result of the increase in population than it was the result of enclosures.[25]

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[18] Root, *op. cit.*, pp. 32–42, and *passim.*

[19] Michael Walzer, *The Revolution of the Saints* (Cambridge, 1965), p. 114. H. R. Trevor Roper, "The General Crisis of the Seventeenth Century," in Trevor Aston (ed.). *Crisis in Europe* (New York, 1967), pp. 99-100.

[20] W. E. Tate, *The Enclosure Movement* (London, 1967), pp. 143-153, and Wilson, *op. cit.*, pp. 141-142.

[21] Some of the important works on the subject include: E. L. Jones and G. E. Mingay (ed.), *Land, Labour and Population in the Industrial Revolution* (London, 1967); E. L. Jones, *Agriculture and Economic Growth in England 1650–1815* (London, 1967); J. D. Chambers and G. E. Mingay, *The Agricultural Revolution, 1750–1850* (London, 1966), F. M. L. Thompson, "The Social Distribution of Landed Property in England Since the Sixteenth Century," *Economic History Review*, XIX, 3 (1966), pp. 505–517. A classic and much earlier "revisionist" analysis is J. H. Clapham, *An Economic History of Modern Britain* (Cambridge, 1926), I, pp. 98–142. See also B. H. Slicher Van Bath, *The Agrarian History of Western Europe* (London, 1963), pp. 318–321. The edited volumes contain some of the classic earlier studies which are the basis for much of the revisionists' case.

[22] Chambers and Mingay, *op. cit.*, pp. 91–92.

[23] Clapham, *op. cit.*, p. 114.

[24] Chambers and Mingay, *op. cit.*, pp. 92–93.

[25] The classic essay is J. D. Chambers, "Enclosure and Labor Supply in the Industrial Revolution," reprinted in Jones, *op. cit.*, pp. 94–127. A recent detailed study by Lawrence White, concludes that there is absolutely no significant relationship between enclosure and migration to the city, or between enclosure and increases in poor rates. The study was done on the basis of an analysis of all English counties, using regression analysis. See Lawrence White, "Enclosures and Population

Further, they argue, all the evidence we have indicates that living standards for workers, both rural and urban, did not fall during the period of industrialization. Rather the picture is one of gradual amelioration interrupted by such things as the effects of the Napoleonic wars, bad harvests, et cetera. On the whole, real wages increased at about the same rate as the relatively slow growth of per capita national product. The evidence indicates that income may have been becoming rather more equally distributed among social classes during the whole period of industrialization. Certainly income inequality does not seem to have increased.[26]

To be sure there was a good deal of suffering among certain groups as old skills became outmoded, or, especially in the south, because of a lack of labor mobility. Certainly the transition might have been made more easily and more humanely in a perfect world. But, the fact of the matter is that the great growth of urban centers and industrialization took place first in England, and in the early nineteenth century. It must be judged by these standards. The alternatives were mass starvation and migration as in Ireland or land hunger and migration to cities in which no jobs were available as in France. In both of these countries population increased during the eighteenth century, but industrialization did not absorb excess labor.[27] Moore conveniently forgets to point out that in France migration to the cities occurred without the benefit of massive enclosures.

Moore agrees with the revisionists that the dismal picture painted of the results of enclosures and early industrialization by radical historians was somewhat overdrawn, but insists, without offering any new evidence, that the radical case still stands in its broadest outline. His critique of the revisionists rests on three kinds of arguments which may, for want of better terms, be called "adjectival," "rhetorical" and "psychological." The "adjectival" argument turns on the tendentious use of key words. Thus when revisionists cite the lack of peasant resistance to enclosure in the nineteenth century as evidence for the fact that peasant hostility to enclosure was not that great. Moore, citing them, states that ". . . the old peasant community finally gave way and disintegrated." (28.) Or where some commentators might see the poor laws and other measures as a sign of the relative responsiveness of the English ruling classes to changes which were but imperfectly understood, Moore emphasizes the horrors of being forced to receive public assistance. He neglects to point out, of course, that however many people were on poor relief in the late eighteenth and early nineteenth centuries, evidence for the late seventeenth and early eighteenth centuries indicates that

> At a conservative estimate, a quarter of the population could be regarded as permanently in a state of poverty and underemployment if not of total unemployment. This was their chronic condition. But when bouts of economic depression descended, the proportion might rise to something nearer a half of the population.[28]

The rhetorical argument is simply that no matter how it occurred, the fact is that the peasantry was destroyed. An entire social class was eliminated. The psychological argument is tied in very closely with the rhetorical argument and is best summarized in Moore's own words:

> . . . it still seems plain enough that, together with the rise of industry, the enclosures greatly strengthened the larger landlords and broke the back of the English peasantry, eliminating them as a factor from British political life. From the standpoint of the issues discussed here, that is, after all, the decisive point. Furthermore, for the "surplus" peasant it made little difference whether the pull from the towns or factories was more important than the push out of his rural world. In either case he was caught . . . between alternatives that meant degradation and suffering, compared with the traditional life of the village community. That the violence and coercion which produced these results took place over a long space of time . . . must not blind us to the fact that it was massive violence exercised by the upper classes against the lower. (28–29.)

This passage is based on a mis-statement of the facts, in so far as it ignores population growth and other causal factors. However, let us disregard that and analyze the other components of the argument.

First, to assert that the elimination of a class and its occupation is by itself evidence of "mas-

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Movements in England, 1701–1831." It will be published in a forthcoming issue of *Explorations in Entrepreneurial History*.

[26] The best summary of the data is to be found in Phyllis Deane and W. A. Cole, *British Economic Growth, 1688–1559* (Cambridge, 1967, 2nd ed.), pp. 25, 27–28, 283, 301, and Lee Soltow, "Long Range Changes in British Income Inequality," *The Economic History Review*, XXI (April, 1968), p. 1729. Needless to say the issue is still fairly controversial.

[27] C. E. Labrousse, *La Crise de l'économie française à la fin d'Ancien Régime et au début de la Révolution* (Paris, 1943), xxii-xxiii.

[28] Wilson, *op. cit.*, p. 231.

sive violence" is to blur key distinctions. Isn't it important whether the change took place suddenly or over a longer time? And doesn't it make a difference for peasants whether they were driven to the city or attracted to it? Common sense, I think, would urge that these considerations *are* of significance if we are concerned with the human costs of social change. According to Moore, after all, industrialization was both necessary and inevitable, and as with all major social changes it involved a good deal of pain. We can evaluate the relative costs in a moral sense (if at all) only in terms of the alternatives which were available at the time, and by comparison with other societies.

Migration to the cities undoubtedly involved degradation for some. That it took place gradually, and that peasants often went to cities not far from their homes probably made the transition less severe. Moreover it was younger people who most often left and they may well have gone to the city because of the wider opportunities it offered.

It is true that, in some cities at least, death rates probably rose with the initial spurt in population, but as Englishmen learned to cope with problems of sanitation, urban mortality began to fall rapidly, and continued to do so during most of the nineteenth century.[29] I suspect that this was a net psychological gain for all city dwellers. Further, there is at least some evidence that by the early nineteenth century, city dwellers were taller and healthier than their rural counterparts, as even a Marxist like Hobsbawm must admit.[30] The disorganization and despair supposedly characteristic of the new working class is based on unproved suppositions, and there is at least some evidence on the other side. Crimes of violence declined in England during the nineteenth century and sporadic riots were increasingly replaced by organized political and trade union activity.[31] Isn't this perhaps a sign that leaving a condition which Marx described as "idiocy," the ability of these peasants turned workers to shape their own destiny was increasing?[32] Isn't it also possible that the relative peacefulness of political activity on the part of the working class was related to gradually improving conditions, and to the general sense of responsibility which, as Moore sometimes admits, characterized both the English aristocracy and the English middle classes?

One more issue deserves consideration in dealing with England, both for its intrinsic importance and for later discussion. Moore is forced to ask himself why English capitalists did not have to engage in the kind of repression during the nineteenth century which he will describe as characteristic of other nations, and thus did not have to turn to the aristocracy for assistance:

> Other capitalist tasks, such as the further disciplining of the labor force, English industrial leaders could carry on their own with a minimum of help from the state or the landed aristocracy. They had to do so because the repressive apparatus of the English state was relatively weak, a consequence of the civil war, the previous evolution of the monarchy, and of reliance on the navy rather than on the army (32.)

One detects here a conservative streak in Moore. It is almost as if institutional and cultural patterns once established have a life of their own. Indeed, to use Moore's own arguments, there is no reason that British industrialists should not have created a repressive apparatus if they felt the need for it. Actually Moore sometimes is forced to admit that conditions in England were improving during the nineteenth century, an improvement he ascribes to English industrial pre-eminence and imperialism, and also to "a strong current of *opinion* in favor of doing something to alleviate mass distress . . . which is traceable to England's historical experience. . . ." (34.)[33]

Despite Moore's rhetoric we have come full circle. In fact, English developments were relatively peaceful, going back even as far as the Civil War, and English industrialization, given the era and the options, was relatively humane.

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[29] See Deane and Cole, *op. cit.*, p. 131 and *passim*.

[30] E. J. Hobsbawn, *The Age of Revolution, 1789–1848* (New York, 1962), p. 28. His data is from Belgium, but one suspects that it applied equally well to England.

[31] J. J. Tobias, *Crime and Industrial Society in the 19th Century* (London, 1967), pp. 22–50.

[32] This is certainly the impression left by E. P. Thompson's *The Making of the English Working Class* (New York, 1966), despite his attempt to prove rather a different kind of case. See, for example, pp. 830–831. We have some hard data on this from contemporary developments. Alex Inkeles, in an intensive study of urban migrants in Chile, and other countries, argues that the adjustment from rural to urban life and to the factory is not nearly so difficult as is sometimes imagined. Indeed urban factory workers from rural backgrounds not only regard their situation as an improvement over rural conditions, but, on most indices which have been used, are more able to cope with the world. The results of Professor Inkeles' work will be published in book form in the near future.

[33] The emphasis in the quote is mine. At one point much later Moore literally brings in the kitchen sink and relates British attitudes to England's whole historical tradition including the use of unpaid justices of the peace. See *Origins*, 444.

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To Moore the French Revolution destroyed a feudal regime which was slowly changing and created the basic institutions of a bourgeois capitalist society. (105–06.) The Revolution took the form it did because, for reasons which Moore does not really explain, the French gentry while becoming capitalist, directed their energies to milking the peasants rather than driving them from the land. (52.)[34] Whatever the costs of the Revolution it was necessary, even in its radical phases, for it produced a society superior by far to the old regime and prevented France from eventually going through a Fascist phase. (78, 104–5, 413.)

As with his English case study Moore's analysis is essentially "pre-revisionist." It is quite clear for example that by the latter part of the eighteenth century, only remnants of what might be loosely called feudalism remained in France.[35] It is also quite clear that the supposedly sharp line between bourgeois and aristocrat was actually far more permeable than has been thought. More important, there is little evidence of a sharp differentiation between the aristocracy and the bourgeosie in their use of capital, their relation to the land, or their participation in commerce. Both groups, if we use the term loosely, were participating in "bourgeois" economic activities in much the same way, and, with exceptions, the economic activity of both groups combined some elements identified with a "capitalist" mentality as defined by Weber, and a pre-capitalist mentality.[36] Finally, it is quite clear that, at least in its early stages, the so-called bourgeois revolution was not initiated by the bourgeoisie, if we define the bourgeoisie in a narrowly economic sense. The leaders of the first moves consisted not of men of commerce but of professionals such as lawyers and bureaucrats of both a "bourgeois" and an aristocratic background. True, the bulk of the initiators were bourgeois, but only in the legal sense that they were not members of the aristocracy, i.e., the sense in which the term was used in the eighteenth century.[37]

Moore admits some of this, and sometimes argues that the French Revolution was a bourgeois revolution not because it was caused by the bourgeoisie, but rather because it established a "capitalist" system of private property, and equality before the law. (105–06.)[38] This argument begs the basic question, i.e., the role played by changes in the system of production. Indeed, if the role of economic classes is not decisive, Moore's whole analysis falls apart, for then landed, non-commercial gentry can make a bourgeois revolution, and Fascism is not the inevitable outcome of the failure of the bourgeoisie to do so. Indeed Moore recognizes this. Throughout the chapter, as in other chapters, the emphasis is upon economic classes as the source of social change, except where it is inconvenient. (68–9 *passim.*)

It is impossible to know whether the terror was necessary for the partial victory of the "bourgeois revolution." *Pace* Moore, the radical phase of the Revolution contributed little to those basic changes in France's legal structure initiated from 1789–1791 and brought to fruition under Napoleon.[39] On the other hand the terror was one of a series of mechanisms by which the Revolutionaries were able to defeat their enemies at home and abroad.

It is also impossible to know whether a milder revolution or non-revolutionary reform would have resulted in a more pronounced "Fascist" phase in French life than she experienced. Moore's argument here depends upon his categories rather than any evidence he adduces. It might be well to point out, however, that while the Revolution did modernize the French legal system and uproot the old class system, paving the way for a more modern society, some of its consequences inhibited later "capitalist" development. The very fact of Revolution and Napoleon's continental system were such that France had fallen much further behind England in terms of economic development by 1815 than she had been in 1789; some promising moves in the direction of free trade taken by the monarchy were dropped by the revolutionaries and Napoleon, and, more importantly, the creation of a large vested group of small peasant owners inhibited rather than aided the growth of commercial agriculture. Nor is there much evidence that the Revolution changed the attitudes of the commercial strata in the society. In fact, the mutual antagonism which characterized various social groups in the aftermath of the Revolution and the instability of French regimes, may have inhibited the development of a more dynamic entrepreneurial attitudes. In any event, French economic growth after 1815 was not any faster than under the old regime, and France did not begin to modernize until the late nineteenth century. In fact her economic structure remained highly traditional until the mid-twentieth century.[40] Nor did the Revolution really solve one of the major problems that had served to bring it about, i.e., the pressure of population on land. This problem was really solved (if this was a solution) by the peasants themselves, as they increasingly turned to birth control and the limitation of family size, a trend which had already manifested itself in the 1770's.[41]

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[34] His supposed explanation actually consists of a description of the differences between English and French gentry.

[35] Alfred Cobban, *The Social Interpretation of the French Revolution* (Cambridge, 1964), pp. 25–53.

[36] See Taylor, *op. cit.*, and Albert Goodwin, "The Social Structure and Economic and Political Attitudes of the French Nobility in the Eighteenth Century," in Comité International de Sciences Historiques, XII, Congrès International . . ., *Rapports*, I, (Vienna, 1965), pp. 356–367.

[37] Cobban, *op. cit.*, pp. 54–67. See also Elizabeth L. Eisenstein, "Who Intervened in 1788? A Commentary on the Coming of the French Revolution," *American Historical Review*, LXXI (October, 1965), pp. 77–103, and the debate which followed in Volume LXXII of the same journal, pp. 497–522.

[38] If establishing equality before the law and a modern system of private property are the essential features of bourgeois democracy, then the restoration in Japan established a bourgeois democratic regime, something which Moore denies.

[39] The definitive study is Jacques Godechot, *Les institutions de la France sous la Révolution et l'empire* (Paris, 1951).

Finally, in addition to the terror, French military casualties during the Revolution and the Empire are estimated at about 1,300,000 men, to which must be added those who died during famines which resulted from general disorder and military action.[42] To all of this we must add the schism in French life produced by the Revolution—a schism which was at least partially responsible for the violence which characterized French life through most of the 19th and early 20th century, as well as the rapidity with which France collapsed during World War II. When Moore asks us whether we would have preferred "moderate reform" to the Revolution, he neglects these results although he is quite willing to place in the balance all of the injustices of the old regime from time immemorial. (104.)

Even those who reject his neo-Marxism would agree that a Revolution like that which developed in France could not have occurred much before 1789. The real question, then, is, given a choice, which alternatives would have been the least costly? In abstract terms, it seems to me that gradual reform on the English model has much to be said for it.

Moore's analysis of the Revolution is unconvincing in its major dimensions. I certainly would not deny the role of the economic changes which were taking place in France in creating pressures for a restructuring of the society. Perhaps my major disagreement with him lies in my willingness to consider as key factors the ideas of the Enlightenment, and the example of England, as these undermined the legitimacy of traditional French society. It was this conjunction of ideological and cultural variables, together with economic changes, which was responsible for the unique development of modern Europe.[43]

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Moore's scattered references to Germany are designed to demonstrate that the failure of a bourgeois revolution and resulting modernization from above led to National Socialism. He never tells us why this should have been so, aside from a few remarks on the irrationality of the German economy. (442.) nor does he satisfactorily explain why the German bourgeoisie failed to create the kind of modern capitalist state which would not have succumbed to totalitarianism.

According to Moore, the bourgeoisie needed the aristocracy if Germany were to be unified and the German working class kept in its place. Not only did they support the aristocracy; they also adopted their life style and values. (36–7, 34, 437.) But why couldn't the German middle classes have supported the aristocracy and retained the "bourgeois" values which their economic position should, theoretically, have impelled them to adopt? By what mechanism is the rational acceptance of the leadership of one class by another transmuted into the acceptance of their values? Why could not the German bourgeoisie merely have accepted the leadership of the aristocracy, until the state had been unified and they were no longer needed, and then dispensed with their services?[44] It is at points like this that Moore's neo-Marxism becomes vulgar Marxism and begins to lose all theoretical coherence.

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[40] For references on this point, see the sources cited in footnote 37. See also Cobban, *op. cit.*, pp. 68–80, Alexander Gerschenkron, "Reflections on Economic Aspects of Revolutions," in Harry Eckstein (ed.), *Internal War* (New York, 1964), pp. 180–204, and Arthur L. Lunham, *The Industrial Revolution in France* (New York, 1955), pp. 3–13.

[41] Wesley D. Camp, *Marriage and the Family in France Since the Revolution* (New York, 1961), p. 101.

[42] Marcel Reinhard, "Bilan démographique de L'Europe, 1789–1815," Comité International, *Rapports, op. cit.*, pp. 451–455.

[43] The classic study of these influences on the revolution is Daniel Mornet, *Les Origines intellectuelles de la Révolution française* (Paris, 1933).

Moore's discussion of Germany flows into his American chapter. In summary his argument is that in 1861, the United States made a choice between an essentially English pattern and a German (or perhaps Russian or Chinese) one. The alternatives were revolution (the Civil War was the "last capitalist revolution"), or an alliance of Northern businessmen and Southern "Junkers." (115, 131, 153, 421.) Moore's explanation of why the alliance did not take place is out of step with much of the best recent scholarship on United States history. But it may be more important to ask whether the materials for an alliance even existed. If they did not, Moore has created an interpretation of the Civil War by posing a false pair of alternatives.

No evidence exists pointing to a possible Northern business and Southern Junker alliance at any time before the Civil War. Neither before nor immediately after the Civil War was there a cohesive business community drawn into supporting common policies by a need to control or a fear of a "radical" working class. Businessmen split widely on every major issue that arose during the whole period, from banking to cheap money, to the tariff, the last of which is central to Moore's argument.[45]

The dual program of cheap land and high tariffs which, according to Moore, forged the economic alliance between the North and the West owed a good deal to the attempt by the Republican party to develop a majority by appealing to a wide variety of groups on the issues which were most salient to them. Moore finds it strange that Northern business never thought to marry the issues which they were supposed to be concerting to marry, and that it was left to "politicians and journalists" to do so. (125–28, 130.) It is only curious if one assumes that political parties are merely epiphenomenal, and that all social policies must be explained in purely economic terms.

The one constituency which supported both free land and higher tariffs was not Northern business, but rather the same elements who provided the "radical" core of the Republican party —small businessmen and farmers. Both groups were wedded to the idea of building a powerful America composed primarily of small property owners, and also saw both the tariff and cheap land as contributing to these goals.[46]

Moore's attempt to differentiate between the North and the South on the basis of their economic systems (contrasting Capitalist systems) is no more successful. It is true that slavery was partly an economic institution, and that the origins of slavery must be sought in economic variables. But unless one assumes that slaves were notably more efficient than free labor, or that only Negroes could effectively work plantations, the fact that Negro slavery came to characterize Southern agriculture, and that the South fought so hard to retain it, cannot be explained by economics alone. On purely economic grounds, there was little reason for southern Americans to refuse to accept the kind of sharecropping arrangements which Brazilians found relatively easy to consider and which, in fact, they ultimately came to themselves in the aftermath of emancipation. In short, racism was a key variable.[47]

We may ask, too, how closely the southern plantation owner resembled the Junker. While social mobility in the South was probably lower than in the North, class lines in the white community were considerably less sharp than in Europe. Indeed the South had accepted universal white suffrage, and Southern politicians had to take into account this constituency. The presence of the blacks also served to reduce social class distinctions between whites.[48] Further, as Moore points out, the Junker used the German peasant as an ally in order to maintain his position. (38.) If the Negro was, as Moore also argues, the only group in the United States resembling a conventional European peasantry, it is extremely difficult to conceive of an alliance of this kind ever developing. (111.)

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[44] We may also ask why the German working class was so much more of a threat to the German bourgeoisie than the English working class was to the English middle class? Despite the rhetoric, they were not notoriously more violent. And if the relatively mild rhetoric of British working class leaders is to be explained by rising living standards, as Moore suggests, why didn't this have the same effect on German workers whose real wages almost doubled between 1870 and 1914? See Ashok V. Desai, *Real Wages in Germany, 1871–1913* (Oxford, 1968), p. 36.

[45] Robert Sharkey, *Money, Class and Party,* Baltimore, 1967), discusses the Reconstruction period in some detail and reviews the general literature. The book is but one of a series which have succeeded in demolishing Charles Beard's portrait of Reconstruction beyond repair. On the Tariff question see Richard Hofstadter, "The Tariff Issue on the Eve of the Civil War," in Gerald D. Nash, *Issues in Economic History* (Boston, 1964), pp. 50–55, and David Montgomery, *Beyond Equality* (New York, 1967), pp. 64–65. On the bank issue see Bray Hammond, *Banking and Politics in America from the Revolution to the Civil War* (Princeton, 1954).

[46] This is the general thrust of W. R. Brock's analysis in *An American Crisis: Congress and Reconstruction, 1865–67* (New York, 1966).

[47] Winthrop D. Jordan, *White Over Black* (Chapel Hill, 1968). Jordan proves fairly conclusively that negative attitudes toward Africans preceded the establishment of slavery, and were a key factor justifying it in the minds of English settlers.

Finally, if it was the North's economic system which produced anti-slavery feeling, one would expect to discover some correlation between type of economic activity and abolitionist sentiment. Moore tries to find this in the family farm in the North (although he is not sure why the same feelings are not produced by family farming in the South). (129.) The evidence belies this. Opposition to the extension of slavery among yeomen farmers in the North was often based on anti-black feeling.[49] On the other hand, if we look for the sources of abolitionist sentiment, the key factor seems to be religious. Abolitionists come in substantial numbers from radical Protestant backgrounds.[50]

The assumption that Northern businessmen would have deferred to the leadership of Southern "Junkers" to produce a "German" solution to industrialization if the Civil War had not occurred is absurd. It does not tie in with our picture of the South or of the country in general. Did the North need the Southern Junker in order to create a national state? Did Northern businessmen emulate the life style or culture of the Southern gentry? On the contrary it was they who felt self-confident and the Southerners who felt on the defensive. One of the reasons that the Civil War came when it did was that Southern militants correctly believed that the balance of power was tipping rapidly in favor of an increasingly powerful industrialized North, and that, given ten or twenty years more, the South would be overwhelmed with ease. To talk of a German solution to industrialization in America is a little like assuming that it is the tail which wags the dog.[51]

Nor is Moore on firmer ground in arguing that Reconstruction was liquidated by the creation of an alliance between Southern ex-Junkers, and northern businessmen, so fearful of nascent radicalism in the North that they were willing to leave the Negro to his fate. Woodward, whom Moore cites for this, offers little or no proof of the assertion in his book, which deals with the compromise of 1877. Rather, a reading of the volume indicates that the compromise, aside from particular political-economic gains for both sides, was forged largely becaused of a fear of renewed sectional violence.[52]

In fact, the evidence points the other way. The Republicans abandoned the Negro because they had become self-confident enough to believe that they no longer needed his votes, and because the passions aroused by the Civil War had died away.[53] The failures of Reconstruction are to be layed to the continuing racism which characterized American society, and to the limitations of the very liberal ideology which was so instrumental in producing the crusade against slavery.[54]

## IV. THE ASIAN CASE STUDIES

While Moore recognizes some of the difficulties of comparing European and Asian countries, he is convinced that the categories he has developed are universally applicable and his discussions of China, Japan, and India are intended to round out both his theoretical and his moral case.

China's failure to break through to a modern society, Moore argues, stemmed not from cultural limitations or institutional structure but from concrete class interests. The Chinese gentry, rather than turning their efforts to raising productivity, preferred to increase their wellbeing by squeezing still more out of the peasantry. (179–80.) In the last analysis the only alternative to a Communist revolution was a quasi-Fascist road to modernization, although, because China was at a relatively early stage of development, this did not take quite the same form as it did in Europe. (196–7, 178.)[55]

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[48] See the discussion in W. J. Cash, *The Mind of the South* (New York, 1941), pp. 31–43.

[49] Eugene H. Berwanter, *The Frontier Against Slavery* (Urbana, 1967).

[50] David Donald, *Lincoln Reconsidered* (New York, 1961), pp. 19–36.

[51] On the relative position of the North and South see Peter d'A. Jones, *The Consumer Society* (Baltimore, 1965), pp. 127–128. That the South faced a crisis of which this factor was part, is pointed out by Eugene D. Genovese, *The Political Economy of Slavery* (New York, 1967). Nevins certainly implies that this was one of the reasons for the militancy of at least some Southerners. See the selection from his *Ordeal of the Union*, Vol. I, in Edwin C. Rolywine, *The Causes of the American Civil War* (Boston, 1961), pp. 200–216.

[52] Indeed Woodward himself lists this as the key factor in the compromise. See C. Vann Woodward, *Reunion and Reaction* (Boston, 1951), pp. 13–14.

[53] Sharkey, *op. cit.*, points out the difficulty of uncovering a unified capitalist versus agrarian or worker position during the period. He is supported in this by Montgomery, *op. cit.*, and Irwin Unger, *The Greenback Era* (Princeton, 1964). Both of the latter stress the importance of ideological as well as economic variables.

[54] See Brock, *op. cit.*, especially pp. 274–304. See also Stanley P. Hirshon, *Farewell to the Bloody Shirt* (Bloomington, 1962), and Kenneth M. Stampp, *The Era of Reconstruction* (New York, 1966), especially pp. 186–215.

Moore's strictures against arguments which explain some of the differences between the West and China on cultural grounds are quite similar to those which he has used before. There is evidence that by the nineteenth century the Chinese were willing to turn to Western technology; the gentry were willing to engage in commerce, and all the right attitudes were available, though perhaps latent. (179.) Ultimately, however, the landed gentry controlled the state, and it was easier to increase the population and exploit peasants than to turn to methods designed to expand production. Why it should have been easier to operate in this fashion, Moore does not tell us, except to assert that ". . . there was no rapidly growing urban population with at least moderately diffused and increasing prosperity that could act as a stimulus to rationalized production for the market." (*Ibid.*)

The argument is unconvincing. Urbanization and moderately diffused prosperity may be a requisite for economic growth (the English gentry, then, were not that exploitive), but China had had both at various periods, including what, in some ways, was a highly developed system of commerce.[56] It is hard to escape the feeling that cultural as well as institutional factors played a significant role in inhibiting Chinese development. Mary Wright, whom Moore cites to support his view that the restoration monarchy could not raise adequate revenues lest it alienate the gentry class, actually argues a quite different case. The regime's failure lay basically in its inability to conceive of efficient ways of using available resources because its institutional structure was based on Confucian precepts. The Japanese monarchy could restructure the economy because the loyalty of the gentry was to a given monarchy and a given nation. In China this was impossible because loyalty was to an institutionalized culture of a universalized type. (182–83.)[57] Moore denies that the Chinese gentry differed in kind from other aristocracies. He bases his case on the argument that since land was a key source of wealth, and wealth was a key source of acquiring bureaucratic position, the differences between the Chinese and other gentry were unimportant. (160–65.) But to paraphrase him, this is to confuse morphology with dynamics. In China potential entrepreneurial talent was diverted to the gentry scholar class through the examination system. In Japan, on the other hand, the fact that aristocratic status was hereditary and closely tied to the land may have been an important factor in the earlier development of an entrepreneurial class.[58]

Chiang Kai-shek is both authoritarian and unattractive. It is less clear that he is a proto-Fascist. This is a key point, for Moore clearly uses Fascist as a scare word, evoking images of genocide. (304–05.) Let us try to deal with the issue. First, within the framework of Moore's own scheme, Chiang Kai-shek could not have established a Fascist regime. Fascism, after all, emerges only after conservative modernization has produced a series of fundamental economic crises. China had not yet passed through such a phase. The point may seem a quibble, but it is extremely important, for if the Kuomintang regime was incipiently Fascist, then Fascism is not merely a later result of conservative modernization, and the whole dialectic of Moore's analysis is open to question.

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[55] Moore is a little more cautious. He points out that the Kuomintang resembled European Fascism in a number of interesting ways. However, the general thrust of his argument is clear.

[56] According to Rhoads Murphey, large cities were proportionately more numerous in China than in Europe until the nineteenth century, and until the eighteenth century urbanism may have been higher. He argues that as much as a quarter or more of the population lived in towns and cities of more than 2500 population and perhaps 10 or 15 per cent in cities over 10,000. Most cities or towns of more than 5,000 had well defined commercial or manufacturing districts, and special areas for each important enterprise. These only represent educated guesses, but I gather that the order of magnitude is accepted as correct by most scholars in the field. See, "The City as a Center of Change; Western Europe and China," *Annals of the Association of Geographers*, LIV (1954), p. 354. Incidentally, in dealing with Japan, Moore drops the requirement of "moderately diffused and increasing prosperity." The key factors in Japan were peace and luxurious display on the part of the nobility (*Origins*, 234–235.)

[57] Wright points out first that there were good economic arguments against increasing the tax on land, and, second, that there were other mechanisms available to the government which would not have alienated any major economic groups, but which it did not use. See Mary C. Wright, *The Last Stand of Chinese Conservatism* (Stanford, 1957), pp. 169, 186, 193. Moore and I must have derived a different meaning from the text, for he cites almost the same pages. Wright does not deny that the regime was limited in its power by various interests. The real question, however, is the relation between interest and ideology.

[58] Marion J. Levy, Jr., "Contrasting Factors in the Modernization of China and Japan," in Simon Kuznets (ed.), *Economic Growth: Brazil, India, Japan* (Durham, 1955), p. 523.

What evidence does Moore offer? Chiang was an incipient Fascist, Moore argues, because he led a single party, which relied, in part, on a secret police apparatus; he used force to solve social problems; he was a nationalist; he urged moral regeneration rather than developing "realistic analyses" of Chinese problems, and he failed to discourage industrialization.

What do these arguments add up to? The leaders of many third world nations could be characterized in much the same way, including such diverse types as Bourguiba, Nasser, Sekou Touré, Nkbrumah and Ayub Khan. To describe them all as latently Fascist or Communist would not seem to further political analysis.

Chiang's writings certainly lack detail on economic questions, but so have the writings of a good many modernizing oligarchs before they consolidated power. In fact, the regime did institute a good many legal and economic reforms between 1928 and 1937, and, it did encourage industrialization. Industrial growth in China between 1928 and 1937 is estimated at perhaps six to eight per cent per year.[59] If his regime made a fatal mistake in ignoring the problems of the peasantry, it demonstrated that it was capable of learning. Once on Formosa it initiated and carried through reforms based on the original Kuomintang program.[60]

Chiang Kai-shek had not yet consolidated his power when the Japanese invaded in force, and his emphasis on building a modern military machine was based on a reasonable assessment of both Japanese and Communist intentions. His failure to press land reform was partly related to a feeling that the consolidation of power and national defense had first priority.[61] Of course, his control over local leaders actually decreased during the war itself.[62]

His policy was clearly wrong. However, we do not know what he would have done had he consolidated his power. After all the Communists also subordinated agricultural reforms to the requirements of victory by downplaying collectivization, just as, at an earlier period, they had worked with the Kuomintang despite the "bourgeois" character of that party.

The creation by Chiang of a police state on the mainland and on Formosa can be explained partially by the fact of very real opposition to his policies. On Formosa these policies include the fanatic desire to reconquer the mainland, a desire as pathetic as it is unreal. However, that a man nearing death should cling to the dream which has given his life meaning, says little about what his actions might have been under other circumstances.[63]

Moore's critique of Chiang's emphasis upon the "excessive" individualism of the Chinese and his calls for moral regeneration is rather peculiar, in that he admits that the theme was not merely rhetorical, and is to be found in the writings of Sun Yat-sen. (208.) The same theme of moral regneration finds its echoes, too, among the Chinese Communists, who are also relying upon moral rhetoric and compulsion to eliminate vestiges of "reactionary individualism."

Finally, as Moore points out in citing the evidence for the regime's incipient Fascism, Chiang argued against precipitant action and urged the important of thinking correctly before acting. (198.) However, nothing could be further from Fascist doctrine, which urges the primacy of action over thought.

Whether Chiang Kai-shek could have succeeded in modernizing China at less cost than that exacted by the Communists or even at all, we do not know. However, the description of his regime as incipiently Fascist does not really contribute to our understanding of either him or China.[64]

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[59] For industrial growth during the period see J. K. Chang, "Industrial Development of Mainland China 1912–1949," *Journal of Economic History*, 27 (March, 1967), 56–81. Professor Chang develops his argument more fully in a new book, *Industrial Development in Pre- Communist China* (Chicago: Aldine Publishing Company, 1969). For a discussion of the efforts of the Kuomintang and the difficulties they faced see Fairbank, Reschauer and Craig, *op. cit.*, II, pp. 691–704. Not all of the latter were of their own making.

[60] Neil H. Jacoby, *U.S. Aid to Taiwan* (New York, 1966), pp. 81–82.

[61] Fairbank, Reischauer, and Craig, *op. cit.*, Franz H. Michael and George E. Taylor, *The Far East in the Modern World* (New York, 1956), p. 409.

[62] Tang Tsou, *America's Failure in China* (Chicago, 1963), p. 77 and *passim*.

[63] It is very difficult to get a clear picture of Chiang Kai-shek. Almost all of the writing about him is heavily rhetorical whether hostile or laudatory.

[64] Revisionist analysts of the Communist revolution in China argue, contrary to Moore, that *per capita* food production was actually rising during the period 1900-1950; that tenancy had not in-

Moore's chapter on Japan is a paradigm study of the relationship between "conservative modernization" and the emergence of Fascism. Given a Japanese bourgeoisie which was too weak to assert itself, conservative oligarchs took charge of the modernization process. The industrialization which followed was characterized by repression, with the masses being kept at a subsistence level. After 1925 the evils of the system became apparent to everyone, and Japanese businessmen, unable to create a vigorous internal market, because it would have threatened their exploitive paternalism and their profits, joined with the military to create a Fascist regime. (290–91, 299–300, 3043–5.) The ultimate result was Pearl Harbor. Japan had escaped the horrors of radical revolution only to create greater horrors for herself. As Moore puts it, comparing Japan to China:

> A hundred years from now, or perhaps in much less time, the partial nature of Japan's social and industrial revolution, especially the very limited "revolution" of the Imperial Restoration in 1868, may seem to be the essence of Japan's tragedy.... On the other hand, contemporary Chinese society, despite severe difficulties and setbacks, shows signs of moving ahead. By learning from Soviet mistakes, China could conceivably surpass Russia. (229–230.)

Moore's analysis of the sources of Japanese economic growth in the pre-Restoration period is unconvincing. If, as he argues, Chinese and Japanese landlords were equally exploitive in intention, and neither contributed in any way to such growth, wherein lies the difference between the two societies? Certainly late seventeenth and eighteenth century China, as Japan, was characterized by both peace and luxury; why did nothing comparable to Japanese development occur? Moore's discussion offers us no clues, unless the Japanese landlord was not as clever as his Chinese counterpart.[65]

How repressive was post-Restoration Japan? If Moore wants to make a case for the "repressiveness" of the Japanese process of industrialization, he must demonstrate that the new regime was more repressive than the old, or that it was more repressive than comparable Asian societies of the period.

It is difficult to support either proposition. While Japan's progress until 1930 was uneven, it was moving in the direction of greater openness. That large numbers of people remained wedded to a traditional outlook is beside the point. A legal system had been created which established relative equality before the law by eliminating feudal privileges; both education and social mobility were rising sharply; the masses were slowly being drawn into the political process, and new ideas were reaching ever larger numbers of people through the press and in the universities.[66]

Moore's contention that the great mass of the population benefited little if at all from industrialization is not supported by the evidence. Indeed the only data he offers are references to an article by Ohkawa and Rosovsky, and to Allen's *A Short Economic History of Japan,* to the effect that Japan was forced to import increasing amounts of rice. He also cites Allen to prove that *per capita* consumption of rice fell after 1925. (289.) Actually, he misreads Allen's figures, for the table cited shows per capita consumption of rice rising between 1925 and 1929 and only falling after that year.[67]

It is true, as Allen points out, that Japan did import increasing amounts of rice beginning after World War I, in part, because Japanese agriculture could no longer advance as rapidly as previously within the existing agricultural framework. However, Allen adds, (and Moore fails to mention), that (1) Japanese population was increasing rapidly during this period. (2) It seemed wiser to grow rice more cheaply in Korea and Taiwan and diversify agriculture on the mainland.[68] Allen's figures show that rice consumption had been rising steadily since 1880. Since the transition to military rule and Fascism came in 1932, according to Moore, it seems a rash judgment to assert that three years of declining consumption are proof that 50 years of steady progress was a "short range success" which had already begun to display its "dubious side," and that now "the evils of the system had become apparent to everyone," especially when

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creased substantially, except around a few large cities, and that the Communists established their strongest and most important bases in areas of relatively low tenancy. See Roy Hofheinz, "The Ecology of Chinese Communist Success: Rural Influence Patterns, 1923–1945," in A. Doak Barnett (ed.), *Chinese Communist Politics in Action* (Seattle: University of Washington Press, 1969), pp. 3–77. His analysis is confirmed in an exhaustive study by Dwight H. Perkins. See his *Agricultural Development in China 1368–1968* (Chicago: Aldine Publishing Company, 1969).

[65] Compare footnote 56.

[66] Fairbank, Reischauer, and Craig, *op. cit.,* II, pp. 513–578.

[67] The table (X) appears on page 201 of Allen's volume.

[68] G. C. Allen, *A Short Economic History of Japan* (London, 1962, 2nd edition), p. 116.

one remembers that the period of decline was partly the product of a world-wide depression. (289, 297.)[69] Japan's agricultural problem was not different from that of some other nations which lacked a traditional landlord class, nor is it true to assert that Japan's oligarchs were unwilling to create "a vigorous internal market." (290, 298.) As Allen indicates, almost eighty per cent of domestic production was for the internal market.[70]

Thus far, I have only relied on sources which Moore cites. Other materials are available which demonstrate that despite a rapid growth in population, the general standard of living rose for the great bulk of the Japanese population. In fact, it rose about as rapidly as the per capita increase in gross national product for most of the period, and it rose more rapidly for peasants than workers in the period before World War I.[71] While per capita consumption of rice fell between 1929 and 1932, per capita consumption of all foodstuffs did not fall but remained at 2,300 calories a day. As Gleason notes:

> A postwar FAO study reported that when climate, body weights, age and sex composition of the population are taken into consideration, the estimated daily requirement per person in Japan is only 2,300 calories. If we accept this estimate as applying to earlier decades, then we may conclude that by 1926 Japan had reached an adequate level of food intake in terms of calories and that this, rather than population pressure, accounts for the stability of the intake during the following decade.[72]

The Japanese economy faced serious problems in the 1930's, problems which stemmed in part from her social structure, in part from the international environment, and in part from the rapidity with which a country with limited natural resources had industrialized. Nevertheless all sections of the population had benefitted from economic growth.

Moore's assertion that big business was one of the main props of Fascism is equally unsupported. The decade of the 1920's, when business seemed to be moving toward domination, was a period during which Japan moved away from her previously imperialist policies; and during the 1930's business, although split, was less sympathetic to imperialist activities than many other groups.[73]

Moore establishes his case by definition. Since the military elite could not do without big business, then big business must have been part of the coalition which supported Fascism. By this logic, any government which had come to power, except for a Socialist government, would have been the one at least partially engineered by big business, whatever the attitudes of business toward it. The same logic can be, and is, used to prove that the Labor government in England came to power in 1945 as a result of its willingness to enter into a coalition with big business, as did the New Deal in the United States. The argument is on a level with one that asserts that the United States is ruled by clerks because all governments have been forced to rely on them.[74]

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[69] One wonders who "everyone" was, given the fact, as Moore himself notes (*Origins*, 300), that right wing parties received only a small percentage of the vote in the 1936 elections. He fails to point out that relatively moderate pro-system parties captured the overwhelming bulk of the popular vote in 1932, 1936, and 1937, and that left wing parties were also quite small. See Robert Scalapino. "Elections and Political Modernization in Japan," Robert E. Ward (ed.), *Political Development in Modern Japan* (Princeton, 1968), pp. 249–92. Since this essay was originally written, I have come across a very useful article on the subject by George M. Wilson. See his "A New Look at the Problem of 'Japanese Fascism,'" in *Comparative Studies in Society and History,* X (July, 1968) 403–412.

[70] Allen, *op cit.*, pp. 159, 213, 224. Indeed Japanese prosperity in the postwar period indicates that democracy and reform are quite compatible with high profits.

[71] Alan A. Gleason, "Economic Growth and Consumption in Japan," in William W. Lockwood (ed.), *The State and Economic Enterprise in Japan* (Princeton, 1965), pp. 391–444. Gleason presents evidence which indicates that from 1890–1925 real consumption *per capita* rose somewhat more rapidly in Japan than in the United States. Some scholars have recently challenged the traditional picture of Japanese economic growth, claiming that living standards before the restoration were higher than is generally assumed, and, thus, subsequent growth was slower. I am in no position to come to any final conclusion on this, but Rosovsky's suggestion that the overall picture is not radically altered seems reasonable. See Henry Rosovsky, "Rumbles in the Ricefields: Professor Nakamura vs the Official Statistics," *The Journal of Asian Studies,* XXVII (February, 1968), 347–360.

[72] Gleason, *op. cit.*, pp. 402–403. In fact rice consumption per head has continued its decline in the postwar period as the Japanese have shifted to other foods, a pattern which began in the 1920's. It is this shift rather than a sudden decline in living standards which probably explains the data cited by Allen. For example Japanese consumption of meat increased 400% in the period between 1892–1896 and 1923–7. *Ibid.*, p. 401.

[73] For example, William W. Lockwood, *The Economic Development of Japan* (Princeton, 1954), p. 76; Reischauer, Fairbank, Craig, op. cit., II, pp. 510–511; William W. Lockwood, "Japan," in D. A. Rustow and Robert E. Ward (eds.), *Political Modernization in Japan and Turkey* (Princeton, 1964), pp. 129–131.

German Fascism was based on a mass party which did draw upon anti-capitalist sentiment. No such party of any real size developed in Japan. To assert that genocide comparable to the Nazis' destruction of the European Jews did not occur in Japan only because capitalism was not sufficiently advanced seems to me a very dubious proposition. (304.) Dominant religious, racial, and ethnic groups have been committing genocide against other groups under all sorts of regimes down through human history. An understanding of the behavior of the Nazis *viz a viz* the Jews must take into account the whole complex relation between Jews and Christians in Europe.

I see little reason to ascribe Japanese imperialism solely to her peculiar economic problems. It is far more easily explained by the emergence of Japan as a nation in an international environment characterized by imperialist nationalisms, and a nationalist tradition of her own which probably finds its ultimate roots in her relation to China. To be sure, the nationalist drive was exacerbated by Japan's dependence on Foreign markets, given the nature of her industrialization, the depression, and the often hysterical reaction of European nations to supposed threats of inundation by Japanese goods. World War II was a tragedy for Japan but certainly not a greater one than France's imperialist adventure during the Napoleonic period, which, to use Moore's logic, must be seen as a natural outgrowth of the French Revolution.[75]

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Moore's Indian case study is his last, and in some ways his most interesting. India has had no bourgeois revolution, nor has it taken the conservative or Chinese road to modernization. Rather the effort has been to raise the standard of living of the Indian people through moderate social reform within the framework of relatively democratic institutions. Thus far the effort has been less than successful, for growth has been slow and almost universal poverty and degradation are the norm. However, the full story has not yet been written and the future is somewhat open. Moore's chapter tries to determine why India chose the road it did, and attempts to evaluate the policies which have been followed.

Moore's analysis of traditional India is based on an examination of the Mogul Empire. He concludes that there were no real signs of incipient industrialization before the British arrived, and leaves the strong impression that Indian society was both far less developed and far more exploitive than either China or Japan. He attributes its failure to develop a surplus to two factors. First, the property of all members of the society could revert to the sovereign upon the death of its owner. The result was an emphasis upon lavish display during one's lifetime. Second, Mogul taxation of the peasant was a fixed proportion of the crop, rather than, as in Japan, a fixed assessment on the land, a practice which discouraged effective cultivation. (328, 333.)

Moore gives relatively little weight to religion or the caste system as a block to development. Caste did render central government superfluous, and inhibited the formation of an alliance of dissident aristocrats and peasants comparable to that which developed in Japan. (353.) However, this was, in the last analysis, of secondary importance. (355.) As for caste itself, its ability to survive for so long is probably due to its very diffuseness, and its origins are to be found in human nature:

> Human beings in a wide variety of civilizations have an observable tendency to establish "artificial" distinctions. . . . Children elaborate artificial distinctions all the time in Western society. . . . The reason for this tendency toward snobbishness—highly developed in some of the most "primitive" societies—is not easy to perceive. Though I cannot prove it, I suspect that one of the few lasting and dependable sources of human satisfaction is making other people suffer and that this constitutes the ultimate cause. (338.)

While the British presence was not as exploitive as some Indian nationalists have charged, it inhibited development. The British imposition of law and order favored those who had property; their attempts to create an independent class of peasant farmers failed; they did not turn economic surpluses into industrial plant; and, most important, their presence precluded both the Japanese and Chinese approach to modernization. (345–47.) Rather, energies which might have been turned in other directions were fused in a nationalist attack upon British rule. The fusion took place in the person of Gandhi, who was able to unite the urban bourgeoisie and the peasantry into one movement. Both he and the Congress Party appealed to commercial interests because his attacks on the British and thus the landlord class which their rule benefitted, were combined with support of the institution of private property. (371, 373, 377, 378.)

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[74] It could also be used to prove that Japanese democracy today is based on big business for the same reasons.

[75] In fact, given the population and weapons differentials which characterized the nations and the periods, it may have been rather less of a tragedy if we, as Moore seems to, take the number of people killed as our index.

Since independence, India has made little progress. Relying on Gandhi's ideology, the government has emphasized programs based on the "felt needs" of the peasants, and has not broken the control of the upper classes in the villages. (392–3.) The fault lies primarily with Nehru, who refused to act decisively although he had much more political leeway than is commonly assumed. (407.) For the future, growth can be achieved by:

> allowing the upper strata in the countryside free rein, but taxing their profits and organizing the market and credit mechanism in such a way as to drive out the moneylender. If the government in this way succeeded in tapping the present surplus generated in agriculture and encouraging the growth of a much bigger one, it could do a great deal more about industry on its own resources. (409.)

Alternatively the government could:

> . . . go over to a much wider use of compulsion, more or less approaching the communist model. Even if it could be tried in India, it seems highly unlikely that it would work. Under Indian conditions for a long time to come, no political leadership—no matter how intelligent, dedicated and ruthless—could . . . put through a revolutionary agrarian policy. The country is too diverse and too amorphous still. . . . The administrative and political problem of forcing through a collectivization program against the barriers of caste and tradition in fourteen languages seems too formidable to require further discussion. (409.)

In conclusion:

> Either masked coercion on a massive scale . . . or more direct coercion approaching the socialist model will remain necessary. (410.)

For, as Moore argues at the beginning of the chapter:

> Political democracy may seem strange in both an Asian setting and one without an industrial revolution until one realizes that the appalling problems facing the Indian government are due to these very facts. (314.)

Moore's analysis neglects almost completely one aspect of India's historical development which differentiates it sharply from China and Japan—the fact that India has yet to develop what both of these countries developed a long time ago, social and political cohesion on a national scale. Historically, the very heterogeneity of the sub-continent must be regarded as an important source of her relative lack of development.[76] India has known only one period of political unity, and the relatively short lived Mogul Empire never covered the whole country. Even within the boundaries of the Empire the administrative unity developed cannot be compared to that of China.[77] If Moore is willing to cite the diversity of India as a serious impediment to any program of reform today, it certainly must be included as a key factor in the past. Nor does Moore touch at all upon ecological problems—India's resource base and the problems of a tropical agriculture dependent upon the monsoon. While such variables are less and less important in the modern period, it seems unreasonable not to deal with them in describing differential development in a pre-industrial past.[78]

Moore's discussion of the origins and development of caste is equally unsatisfactory. The fact that caste is eroding today under the impact of economic change says nothing about its effect on economic development at an earlier period, and Moore's analysis of its origins in the cupidity of human nature fails to explain why human viciousness took the peculiar form it did in India. One wonders, too, why the particular method of exploitation characteristic of India, i.e., taking a fixed portion of the crop, should have so inhibited the development of a surplus; after all, the absolute amount available to the peasant would have grown if his productivity increased. Even if Moore's argument is correct, why was the equally exploitive Japanese ruling class so stupid as to fail to see the advantages of adopting Indian customs in determining their share of peasant efforts? One wonders, too, why Chinese rulers failed to latch on to a custom of expropriating wealth with death along the lines followed by Akbar?

The weakness of Moore's argument becomes apparent in his treatment of British rule, for by his own analysis the British eliminated at least some of the mechanisms which had prevented the development of a surplus, and the only result, at least according to the author, was the continued growth of parasitic landlordism. It may be true that the overall effect of the British presence and the emergence of Gandhi, was to inhibit the development of either the Japanese or the Chinese alternative, as Moore argues. However, his case depends upon the acceptance of his categories, rather than on the evidence. It can be just as plausibly argued that if British rule had not provided a common focus for nationalist aspirations, and Gandhi had not appeared, the result would have been the complete fragmentation of the subcontinent. (446.)[79]

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[76] A. L. Basham, *The Indian Sub-Continent in Historical Perspective* (London, 1958).

[77] The point is made by Moreland, upon whom Moore relies quite heavily, See W. H. Moreland, *India at the Death of Akbar* (London, 1920), p. 4.

[78] Morris D. Morris, "Towards a Reinterpretation of Nineteenth Century Indian Economic History," *Journal of Economic History* (December, 1963), p. 609.

I am not prepared to judge the efficacy of Nehru's policies. On Moore's own evidence their weakness lies not in the failure of the regime to break the power of the upper classes, but rather on its failure to develop intelligent programs. Moore does not accurately cite a single case study in which reform was blocked by the power structure of the village. He does cite several in which bureaucratic routine and an excessive idealization of traditional village culture resulted in failure. (400–403.)[80]

Interestingly enough, this is the only case study in the volume in which, at various times, ideological commitment as against class interests is explicitly accorded substantial independent causal efficacy. Thus, British reforms in agriculture are laid to the ideology of the British, and Nehru's failures are laid to an idealization of the village and an unwillingness to rely on certain forms of compulsion.

Why does Moore suddenly reverse himself? Perhaps because India is the prime example of the failure of moderate reform. Here men of relatively good will have attempted to avoid various violent and repressive models, and have failed. Hence, Moore drops his neo-Marxism in dealing with India to make a policy point, namely that neither liberalism nor democracy can work in the developing countries. The lesson to be derived from the Indian case is quite clear. Despite some cautionary statements, Moore believes that the only two alternatives open for India are repression on the Japanese model and violent revolution of the Chinese type. Given his previous assessments of these alternatives, we can guess which he prefers. This brings us to the heart of the assumptions underlying his whole effort.

### V. THE UNDERLYING ASSUMPTIONS

Moore's analysis does not establish either his substantive or his theoretical propositions. All industrialization has not been more or less equally violent and repressive; all ruling classes are not equally exploitive; and Fascism is not the necessary outcome of so called "conservative industralization." If I have justified my initial assertion that Moore's perspectives were brought to rather than deriving from the materials with which he deals, we may now turn to a direct examination of these perspectives.

Toward the end of *Social Origins of Dictatorship and Democracy,* Barrington Moore states some of his assumptions in fairly clear form. The three paragraphs involved are worth quoting in full:

> For a Western scholar to say a good word on behalf of revolutionary radicalism is not easy because it runs counter to deeply grooved mental reflexes. The assumption that gradual and piecemeal reform has demonstrated its superiority over violent revolution as a way to advance human freedom is so pervasive that even to question such an assumption seems strange. In closing this book I should like to draw attention for the last time to what the evidence from the comparative history of modernization may tell us about this issue. As I have reluctantly come to read this evidence, the costs of moderation have been at least as atrocious as those of revolution, perhaps a great deal more.
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> Fairness demands recognition of the fact that the way nearly all history has been written imposes an overwhelming bias against revolutionary violence. Indeed the bias becomes horrifying as one comes to realize its depth. To equate the violence of those who resist oppression with the violence of the oppressors would be disleading enough. But there is a great deal more. From the days of Spartacus through Robespierre down to the present day, the use of force by the oppressed against their former masters has been the object of nearly universal condemnation. Meanwhile the day-to-day repression of "normal" society hovers dimly in the background of most history books. Even those radical historians who emphasize the injustices of prerevolutionary epochs generally concentrate on a short time span preceding the immediate outbreak.

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[79] Moore, in fact, suggests that one of the major reasons for the lack of development of a Hindu variant of Fascism is the fragmentation of the Hindu world along caste class and ethnic lines.

[80] To prove his case, Moore uses Rene Dumont, *Terres Vivantes* (Paris, 1961); Gough's essay in McKim Marriott, *Village India* (Chicago, 1955), and Alan R. Beals, *Gopalpur* (New York, 1962). None of them directly support his assertion and Beals states almost the opposite on p. 82.

In that way, too, they may unwittingly distort the record.

That is one argument against the comforting myth of gradualism. There is an even more important one, the costs of going without a revolution. There have been the tragedies of the victims of fascism and its wars of aggression, the consequence of modernization without a real revolution. In the backward countries today, there continues the suffering of those who have not revolted. In India we have seen that this suffering has been in good measure the price of democratic slowness in the Asian context. . . . There are also positive arguments on behalf of revolution. In the Western democratic countries revolutionary violence (and other forms as well) were part of the whole historical process that made possible subsequent peaceful change. In the communist countries too, revolutionary violence has been part of the break with a repressive past and of the effort to construct a less repressive future. (505–506.)[81]

I do not know on what basis Moore arrives at these generalizations about Western scholars. The best historians are quite willing to describe regimes as exploitive when they feel that they are, and do not merely describe their problems on the eve of revolutions, or only condemn the violence of the "oppressed." They are rather less likely than Moore, however, to set up an absolute standard of exploitation. Rather they are inclined to evaluate regimes more or less within the context of the historical period, and with an awareness of both the cultural and technical resources available. Nor do they usually divide the historical actors categorically into oppressors and oppressed. Rather, they have a sense of history as involving tragic encounters among men equally caught up in their own limitations, and, in evaluating the outcome of revolutions, they attempt to balance losses against gains, and to criticize only violence which seems purposeless or unnecessary.

Moore's writing indicates that he is one of that generation of scholars to whom the Soviet Revolution once represented the hope of a radical transformation of mankind. Disillusioned by the results of that Revolution, he has now, with the mellowing of the Soviet regime, been able to justify his earlier enthusiasm for it by arguing that it has been no more repressive than other alternatives. (31, 506.) And while he is no longer sure that a free non-repressive society can ever be created, he looks to new radical revolutions in the "third world" as embodying hope for the future. (508, and his remarks on China.) Indeed, while he talks of past revolutions in the West as having provided the opportunities for later peaceful change, he seems convinced that Western societies are becoming increasingly irrational, and that change may have to involve the repression of false ideas if we are to move on toward the goal of a compulsionless society. (442.)[82] However, much he may talk about an objective standard of exploitation which may be used for purposes of historical analysis, it is also clear that his definition of exploitation derives from his conception of a free rational and compulsionless society. By this definition, of course, all historical societies have been exploitive, and his refusal to differentiate degrees of exploitation now makes sense. To a man standing on the moon the differences between mountains and valleys on the earth must seem rather insignificant. By these standards, all socialization is repressive.

In all of this, his neo-Marxism serves a useful function. After all, if men can learn, and if new ideas can change their behavior, then peaceful reform is possible. If, on the other hand, class interests deriving from the economic substructure of the society and acquisitive drives are the basic motivations in all historical systems, then change must be apocalyptic. Further, if history is the product of clashes between human beings who are caught up in cultural limitations, and who fight and die for ideas sincerely held, it is far more difficult to dismiss their suffering than if they are merely "exploiters" and "oppressors." The image overwhelmingly conveyed by the volume is that of a Manichean struggle between Good and Evil.

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[81] Moore loves to cite such great "truths" as: "A policy of law and order favors those who already have privileges, including some whose privileges are not very large." (Origins, 354). To which I might reply: "A policy of violence and disorder favors those who are both aggressive and strong, including those who are just a little more aggressive than the vast majority of the people."

[82] Moore is very ambivalent on this question. See his essay in Robert Paul Wolff *et al., A Critique of Pure Tolerance* (Boston, 1965). He is ambivalent on a number of other issues as well. On the one hand he feels that men are capable of discovering scientific truths which will enable them to create a more humane society. On the other he is not sure that enough men can learn quickly enough to avoid the necessity for revolution even in advanced countries. He is not clear as to whether it is their class position, their general selfishness or their ignorance (or stupidity) which is the key variable here. In *Origins* he gives not a single instance of men learning from past mistakes in such a way as to act more humanely for the general welfare, and yet the implication of his chapter on India is that its leaders can learn a limited truth, i.e., the need for some form of compulsion if India is to industrialize.

And yet Moore's neo-Marxism also has disadvantages. Aside from the difficulties of applying the analysis to his material, difficulties which I have tried to outline in this paper, the approach limits his ability to draw moral lessons from history. What moral guide to action can we draw from the Japanese experience? As Moore describes it, the actors moved inevitably to a predetermined end.

Moore uses his categories as a rhetorical device, discarding them when they do not serve rhetorical purposes. Thus to demonstrate the impossibility of liberal or moderate reforms, he sometimes treats liberal or reformist ideas and the men who held them as free agents, who apply their ideology because they mistakenly believe in it rather than because of exploitive class interests.

It may be that violent national Communist revolutions are necessary if a good many of the developing nations are to modernize, and that the costs of such revolutions would be less than the costs of not having them. However, Moore has not made a case for this proposition. I find his position intellectually and morally untenable.

# REPLY TO ROTHMAN

Barrington Moore, Jr.
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Stanley Rothman is laboring under a series of misapprehensions. What appears to give them some minimal coherence is evidently the curious conviction that in criticizing *Social Origins of Dictatorship and Democracy* he must cope with the work of a disillusioned former fellow-traveller. In discussing my alleged fundamental assumptions, toward the end of his essay, Rothman asserts:

> Moore's writing indicates that he is one of that generation of scholars to whom the Soviet Revolution once represented the hope of a radical transformation of mankind. Disillusioned by the results of that Revolution, he has now, with the mellowing of the Soviet regime, been able to justify his earlier enthusiasm for it by arguing that it has been no more repressive than other alternatives. (31,506.)[1]

Upon reflection I think I am entitled to take this as a compliment to my efforts at critical detachment. In my books on the Soviet Union I must have restrained my hostility to the point where my critic could succeed in discovering what he takes to be evidence of enthusiasm. At any rate, his discovery is a completely original one!

After this discovery Rothman goes on to attribute ideas to me that he probably got from reading the works of my very good friend, Herbert Marcuse. For example, so far as I remember, I have never used the expression "non-repressive society," which has become in a way Professor's Marcuse's trade-mark. ("Less repressive" is quite another expression, which I do use.) Nor did I use the expression "free non-repressive society," preferring to say "free and rational society," on the page cited by Rothman in this same paragraph. Indeed, to continue examining this passage as an example of Rothman's criticism, I have never been sanguine about "revolutions in the third world as embodying hope for the future" of mankind, much as such revolutions against American attempts to prop up various forms of political landlordism do seem to me justified. But perhaps Rothman is simply mixed up, because I doubt very much that either Marcuse or I would ever speak about "the goal of a compulsionless society!"

By this point it may already be apparent that the basis of Rothman's critique is so comic I wonder whether he deserves a serious reply. I have decided to say something, however, because there may be a number of readers of this journal seriously interested in the issues who have not read *Social Origins*. It is also quite possible that he and his helpers have found some factual errors.[2]

What appears to be a more damaging accusation is the kind of flat statement Rothman makes about my alleged inability to cite properly. For example, in referring to my discussion of the Community Development Program in India, he says: "Moore does not accurately cite a single case study in which reform is blocked by the power structure of the village." (p. 00) In the accompanying footnote, he cites only three of my many sources, declaring: "None of them directly support

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[1] With a sense of amused astonishment I reread pages 31 and 506 in *Social Origins*, which Rothman cited as specific evidence for his claims. On page 31 in footnote 65 I was *describing* a type of disillusionment that took place in England as the French Revolution entered its radical phase and comparing that disillusionment with American reactions to communist expansion after 1945. In so doing I said: "There is the same ambiguity about the character of the revolutionary enemy, the same exploitation of this ambiguity by the dominant elements in society, *the same disillusionment and dismay among its original supporters as the revolution abroad deceived their hopes.*" (Italics added) How Rothman could so misread my remarks as to attribute this type of disillusionment to me personally I cannot understand. On page 506 there is no hint at all of my alleged shift of views. Perhaps what Rothman meant to cite was page 505 where I said: "As I have reluctantly come to read this evidence, the costs of moderation have been at least as atrocious as those of revolution, perhaps a great deal more."

[2] That I misuse my sources Rothman asserts more than once. Sometimes he means that my interpretation of an author's data leads me to a conclusion different from that of the author, as in the case of my use of the evidence in Mary Wright's *Last Stand of Chinese Conservatism*. That is a valid difference, I believe, and one that Rothman also uses when, for example, he draws different conclusions from those of E. P. Thompson (footnote 32), but its validity rests, of course, on the evidence offered for the change.

This, however, is a matter I must now leave to specialists, since my work for the past five years has been on other problems. On the other hand, Rothman's treatment of my text raises serious doubts about his scholarly craftsmanship.

As a result of Rothman's technique of twisting arguments in *Social Origins* and then attacking these caricatures, the author finds it impossible to recognize his own views. *Social Origins* is mainly about different forms of social structure, their varying origins and political consequences. In its essentials, this central aspect escapes Rothman's vision. Thus he can assert that "Moore denies that the Chinese gentry differed in kind from other aristocracies," and can put the rhetorical question: "If, as he argues, Chinese and Japanese landlords were equally exploitive (*sic*) in intention, and neither contributed in any way to such [economic] growth, wherein lies the difference between the two societies?" After the pains to which I went to describe the differences—in respect to internal organization, relationships to the peasantry, to the government, and to the commercial classes—Rothman's comments show that he chose to sweep these crucial considerations under the nearest rug.

Somewhat earlier in his essay there is an even more glaring example. About my discussion of the American Civil War, Rothman says: "The assumption that Northern businessmen would have deferred to the leadership of Southern 'Junkers' to produce a 'German' solution to industrialization if the Civil War had not occurred is absurd." (p. 72) Here, as in many other instances, Rothman attributes to me views that are the precise opposite of those I expressed. My point was *not* that the Civil War prevented this form of coalition. Rather it was that the Civil War happened in large measure because the conditions necessary for this type of reactionary coalition were weak or absent.

Also typical is Rothman's habit of selecting part of an argument that seems to support his charges and omitting material that shows his claims to be false or ridiculous. For example, in my general discussion on moderate reform and revolutionary radicalism (pp. 505–506), Rothman chooses to quote *only* the three paragraphs that deal with the costs of revolutionary violence. The fourth paragraph, without which the whole discussion loses its focus, begins:

> The gradualist argument seems shattered. But precisely at this point the revolutionary argument also collapses. It is clear beyond all shadow of doubt that the claims of existing socialist states to represent a higher form of freedom than Western democratic capitalism rest on promise not performance. There is no denying the patent fact that the Bolshevik Revolution did not bring liberation to the people of Russia. At most it may have brought the possibility of liberation. Stalinist Russia was one of the bloodiest tyrannies the world has yet seen.

In the same vein I went on to say that "The range and depth of Stalinist repression and terror were far too great to find explanation, let alone justification, through some conception of revolutionary necessity." For this terror, I pointed out, there were deep institutional roots, an issue discussed in other writings of mine. "In general," this passage concludes, "one of the most revolting features of revolutionary dictatorships has been their use of terror against little people who were as much victims of the old order as were the revolutionaries themselves, often more so."

On the whole, for all its scholarly façade, Rothman's critique has much of the air of a Congressional inquiry. Throughout, but especially toward the end, it has the characteristic ring of orthodoxy under threat and engaged in unmasking presumably dangerous intellectual in-

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his assertion and Beals [*Gopalpur*] states almost the opposite on p. 82."

In the first place, it is simply not true that the three sources (and others cited earlier and later) do not "directly support" my statements, which consist mainly of close paraphrases from the sources cited. Now, as to Beals' stating "almost the opposite" of my general view that the wealthy landlords blocked reform when it threatened their interests or control over the village, I see not the slightest contradiction on p. 82 of Beals' study. There Beals credits the wealthy Gopalpur landlords, who are the "kingpin of the social structure of the villages they control," with being "the innovators who introduce new agricultural techniques," as well as other improvements in the village in many cases. But unlike Beals, Rothman does not ask the simple question: "For whose benefit are these innovations or reforms?" Beals had already given his explicit answer on p. 80, where he pointed out that some wealthy landowners introduced new and costly fertilizer, equipment, crops and techniques—but they did this only on their own large acreage farms. Many of these innovations were not suitable for farms of small acreage, as is true elsewhere in the world, of course. Small farmers, then, remained dependent for seed and credit on the wealthy landlord. Furthermore, as Beals adds, on p. 82 but not cited by Rothman, "If those who now serve as laborers are converted into farmers, there will be no one to do the weeding and harvesting." The landlord's stake in the *status quo* is, I think, clear.

tentions. If there *is* a danger somewhere—and there probably is to important values that Rothman may possibly share with me—that danger resides primarily in a set of social facts and trends. *Social Origins* did not create the facts, and Rothman's earnest efforts will not make the facts go away.

As part of such attempts, he calls my views "neo-Marxist" or "vulgar Marxism." Now, my general view of Marxism is that it is often superb for the insights it gives into the behavior of the upper classes, but that it is nearly worthless for the understanding of the behavior of the lower classes. To be just a trifle more specific, the ways in which dominant classes pump an economic surplus out of the lower classes does, in my judgment, constitute a major key to the understanding of any specific social structure. Political levers, traditional customs and even values, all play their part—often one more crucial than the mode of production itself, in this process of extracting the surplus.[3] On the other hand, the Marxist tradition is of little help, I find, in understanding those aspects of social structure among the lower classes that lead at times to resistance and revolution against the prevailing order or at other times to docile acceptance. If that is "neo-Marxism" according to *Social Origins,* so be it. I find only sheer caricature in Rothman's effort to restate my assumptions in the opening portion of his critique, with its remarks about the mode of production determining ideology, or on exploitation as a "primary goal" of ruling classes, or on the survival of Hinduism and the caste system "only because they served the interests of the ruling strata," or on the Meiji Restoration as "based entirely on the self-interest of a particular segment of the ruling class," or on past revolutions as "inevitable" (something patently unprovable). His restatement is a caricature of the *conclusions* I reached after much puzzling and re-thinking—and does not even dimly reflect the assumptions with which I began. In any case, the issue is not whether *Social Origins* is neo-Marxist or not—but whether it is correct.

It would be possible, though fruitless, to go on at much greater length and in greater detail—especially fruitless since "explicit disclaimers" do not constitute legitimate evidence to my critic. The book itself really has to stand as the most valid answer to Rothman's kind of criticism. It is safe to anticipate that my critic can and will find even more ideas in *Social Origins* and in my reply that he can misrepresent. Yet the ambiguity of his last paragraph gives me reason to think that the book carried some persuasion, even for him. Therefore I hope that even he may wish to reread it in a calmer spirit some years hence.

(Editor's Note: For further comments by Professor Rothman, see the Communications section of this issue.)

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[3] As to my being anti-Weber, I can only say that I regret that Rothman can read *Social Origins* as constituting in any substantial sense an attack on Max Weber, whom I greatly respect and whose pessimistic views on Western civilization I share.

# A POSSIBLE EXPLANATION OF ROUSSEAU'S GENERAL WILL[1]

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A "general will" is a philosophical and psychological contradiction in terms; will is a conception understandable, if at all, only in terms of individual actions. The problem cannot be glossed over by attempting to reduce the general will—as did T. H. Green—to a "common ego," or to an analogical forerunner of Kant's pure practical reason.[2] Why, then, did Rousseau make so unviable an idea the center of his political theory,[3] and why has that idea continued to receive serious attention?

The general will has continued to be taken seriously because it is an attempted (though not explicit) amalgam of two extremely important traditions of political thought, which may be called, roughly, ancient "cohesiveness" and modern "voluntarism." Political thought since the 17th century has been characterized, among other things, by voluntarism, by an emphasis on the assent of individuals as the standard of political legitimacy.[4] One certainly finds this in many of the most important thinkers between Hobbes and Kant; and even Hegel, while scarcely an "atomistic individualist" or a contractarian, explicitly argued that while "in the states of antiquity the subjective end simply coincided with the state's will," in modern times "we make claims for private judgment, private willing, and private conscience." When a political decision is to be made, Hegel continued, "an 'I will' must be pronounced by man himself." This "I will," he thought, must have an "appropriate objective existence" in the person of a monarch; "in a well-organized monarchy, the objective aspect belongs to law alone, and the monarch's part is merely to set to the law the subjective 'I will'."[5] If even Hegel allows this voluntarist turn in his own non-contractarian theory, it goes without saying that all of social contract theory can be seen as the supreme example

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N. B.: For the convenience of American readers, reference has been made whenever possible to a standard English translation of Rousseau. All translations from French editions are my own.

[1] This title was adopted not out of modesty, but simply because this article makes certain assumptions which are not universally agreed on. Most importantly, it holds that Rousseau understood "will" not only as a psychological attribute but also as a moral faculty; that there is an implicit metaphysical dimension in Rousseau without which a concept such as "willing" would be incomplete. By treating the concept of volition in psychological rather than in metaphysical terms, however, Mrs. J. N. Shklar, in her persuasive *Men and Citizens: A Study of Rousseau's Social Theory* (Cambridge University Press, 1969) is able to make the general will both internally consistent and closely related to Rousseau's individual psychology. That is, by treating individual will as a defense-mechanism, and the general will as a collective defense-mechanism (used largely as a weapon against inequality, whose effects for Shklar are mainly psychologically destructive), she is able to make the general will conceptually plausible. This reading, however, seems to involve a weakening of those (admittedly few) passages in which Rousseau speaks, in a traditional way, of volition as a moral faculty whose endorsement is the source of all moral legitimacy, e.g., "to deprive your will of all freedom is to deprive your actions of all morality" ( *Contrat Social,* chapter IV). This idea is clearly the foundation of Rousseau's attack on paternalism and on the equation of right and force. It is certainly possible to conceive the "general will one has as a citizen" in a psychological and metaphorical sense, if one imagines this "generality" in terms of factors wholly congruent with psychology: education, public spectacles and games, the authority of the legislator. But this can never show how a will in anything *but* a psychological sense can be general, simply because the education and authority congruent with psychology are *not* congruent with a concept of free will and of moral choice. A psychological treatment of volition does in fact hold out the best prospect of a consistent treatment of Rousseau; but it also involves a weakening of the voluntarist and contractarian elements of his philosophy.

[2] Green, T. H., *Lectures on the Principles of Political Obligation.* (Longmans, Green and Co., London, 1941), pp. 82 ff.

[3] Barth, Hans, "Volonté Générale et Volonté Particulière chez J.-J. Rousseau," in *Rousseau et la Philosophie Politique,* Annales de Philosophie Politique No. 5, (Presses Universitaires de France, Paris, 1965), pp. 44 ff.

[4] Chapman, John, "Political Theory: Logical Structure and Enduring Types," in *L'Idée de Philosophie Politique,* Annales de Philosophie Politique No. 6, (Presses Universitaires de France, Paris, 1965), pp. 65–69.

[5] Hegel. G. W. F., *Philosophy of Right,* translated T. M. Knox, Oxford University Press, London, 1942, pp. 280, 288–289.

of voluntaristic ideas. No theory, then, escaped the enthronement of will—quite literally, in Hegel's case—at some point in the political scheme. Why voluntarism, political legitimacy through authorization by individual wills, came to hold such an important place in Western thought, requires a book to itself, a history of political will.[6] What is probable is that ancient quasi-aesthetic theories of the best regime and the proper end of man gave way, with the introduction of Christianity, to thinking about politics after the model of "good acts": just as good acts required both knowledge of the good and the will to do good, politics now required moral assent, the implication of the individual in politics through his own volition. The freedom to conform voluntarily to absolute standards had always been important in Christian doctrine; the Reformation doubtless strengthened the element of individual choice in moral thinking, while downplaying the role of moral authority. And it was natural enough that the Protestant view of individual moral autonomy and responsibility should spill over from metaphysics into politics, forming the intellectual basis of contract theory. The mere excellence of a social institution would no longer be enough; it would now require rational assent. However voluntarism and contract theory arose, what is certain is that ideas of the "good" state increasingly gave way to ideas of the merely "legitimate" state.[7] And this legitimacy, after the 17th century, was often taken to rest on will.

But while voluntarism took care of legitimacy, it could say nothing about the intrinsic goodness of what is willed. It is, of course, possible to assert that whatever is willed is right simply because it is willed.[8] But this was not enough for Rousseau; and it was precisely here that he made a stand for a particular kind of will. He wanted will to take a particular form; he wanted voluntarism to legitimize what he conceived to be the unity and cohesiveness—the generality—of ancient polity, particularly of Sparta and Rome. Indeed his political ideal was the ancient polity, now willed by moderns who were as concerned with reasons for obligation as with perfect forms of government. Against the alleged atomism of earlier contract theory, Rousseau wanted the generality—the non-individualism, or rather the pre-individualism—of antiquity to be legitimized by consent. Here of course he got himself into the paradox of insisting on the willing of the essentially non-voluntaristic politics characteristic of antiquity. He got himself, that is, into a philosophical paradox of willed non-voluntarism; but if this paradoxical concept, the general will, a will which is the corporate "will" of a whole society, a will to stop being willful, cannot be philosophically defended, it can at least be unravelled with interesting implications for all voluntaristic and perfectionist theories—not to mention democratic theories, which are always hard-pressed to fuse what is wanted with what is intrinsically good. And this treatment helps to clear up some of the usually insoluble paradoxes in Rousseau, and to make his thought clearer, if not less problematical.

## I

Jean-Jacques Rousseau was a severe critic of modern political life—of its lack of a common morality and virtue, of its lack of patriotism and civic religion, of its indulgence in "base" philosophy and morally uninstructive arts.[9] At the same time, he was a great admirer of the more highly unified political systems of antiquity, in which, as he thought, morality, civic religion, patriotism, and a simple way of life had made men "one," wholly socialized and truly political.[10] And he thought that modern political life divided man against himself, leaving him, with all his merely private and anti-social interests, half in and half out of political society, enjoying neither the amoral independence of nature nor the moral elevation afforded by true socialization.[11]

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[6] Plamenatz, John, "Equality of Opportunity," in *Aspects of Human Equality*, edited L. Bryson, (Harper and Bros., New York, 1957), pp. 79–107.

[7] Jouvenel, Bertrand de, *Sovereignty*, translated by J. Huntington, (University of Chicago Press, Chicago, 1957), various passages.

[8] Leibniz accused Hobbes and Pufendorf of adhering to this view. "To say *stat pro ratione voluntas*, my will takes the place of reason," Leibniz observed, "is properly the motto of a tyrant." He traced this line of thought back to Thrasymachus in the *Republic*, and urged that Hobbes, "who is noted for his paradoxes, wanted to maintain nearly the same thing as Thrasymachus." See Leibniz, G. W., *Meditation sur la Notion Commune de la Justice*, edited G. Mollat, Verlag J. H. Robolsky, Leipzig, 1885, pp. 56 ff.

[9] See particularly Rousseau, J.-J., *Discourse on the Arts and Sciences*, translated G. D. H. Cole, (E. P. Dutton and Co., New York, 1950), pp. 172–174; Rousseau, J.-J., *Gouvernement de Pologne* in *Political Writings*, edited C. E. Vaughan, (Basil Blackwell, Oxford, 1962), Vol. II, pp. 430, 437–438; Rousseau, J.-J., *Disease on Inequality*, translated G. D. H. Cole, (E. P. Dutton and Co., New York, 1950), pp. 247 ff., 266 ff.; Rousseau, J.-J., *Lêttre à M. d'Alembert*, critical edition of M. Fuchs, (Librairie Droz, Geneva, 1948) (entire).

[10] See particularly Rousseau, *Gouvernement de Pologne*, *op. cit.*, chapters II and III; Rousseau, *Discourse on the Arts and Sciences*, op. cit., pp. 153–158; Rousseau, J.-J., *Economie Politique* in *Political Writings*, edited Vaughan, op. cit., Vol. I, pp. 253–254; Rousseau, J.-J., *Rome et Sparte*, in

Why Rousseau thought the unified ancient political systems preferable to modern ones is not too hard to understand. He conceived the difference between natural man and political man in very sharp terms; while for most contract theory political life was merely non-natural (and this largely to do away with arguments for natural political authority), for Rousseau it was positively unnatural, or anti-natural, a complete transformation of the natural man. The political man must be deprived of his natural powers and given others, "which are foreign to him and which he cannot use without the help of others"; politics reaches a peak of perfection when natural powers are completely dead and extinguished, and man is given a "partial and corporate existence."[12] The defect of modern politics, in Rousseau's view, was that it was insufficiently political; it compromised between the utter artificiality and communality of political life and the naturalness and independence of pre-political life, and in so doing caused the greatest misfortunes of modern man: self-division, conflict between private will and the common good, a sense of being neither in one condition nor another. "What makes human misery," Rousseau said in *Le Bonheur Public*, "is the contradiction which exists between our situation and our desires, between our duties and our inclinations, between nature and social institutions, between man and citizen."[13] To make man one, to make him as happy as he can be, "give him entirely to the state, or leave him entirely to himself . . . but if you divide his heart, you will rip him apart; and do not imagine that the state can be happy, when all its members suffer."[14]

Above all, the imperfect socialization of modern man, in Rousseau's view, allowed private persons and corporate interests to control other private persons, leading to extreme inequality and personal dependence; only generality of laws based on an idea of common good, he thought, could abolish all private dependence, which was for him, perhaps, the supreme social evil. What he wanted was that socialized men might be "perfectly independent of all the rest, and extremely dependent on the city,"[15] for only the power of the state, and the generality of its laws, "constitutes the liberty of its members."[16]

Ancient polities such as Sparta, Rousseau thought, with their simplicity, morality (or politics) of the common good, civic religion, moral use of fine and military arts, and lack of extreme individualism and private interest, had been political societies in the proper sense: in them man was "part of a larger whole" from which he "in a sense receives his life and being."[17] Modern "prejudices," "base philosophy," and "passions of petty self-interest," on the other hand, assure that "we moderns can no longer find in ourselves anything of that spiritual vigor which was inspired in the ancients by everything they did" (*Gouvernement de Pologne*).[18] And this spiritual vigor may be taken to mean the avoidance (through identity with a "greater whole") of "that dangerous disposition which gives rise to all our vices,"[19] self-love. Political education in an extremely unified state will "lead us out of ourselves" before the human ego "has acquired that contemptible activity which absorbs all virtue and constitutes the life and being of little minds" (*Économie Politique*).[20] It follows that the best social institutions "are those best able to denature man, to take away his absolute existence and to give him a relative one, and to carry the *moi* into the common unity" (Émile).[21] These social institutions, in ideal ancient polities, were always for Rousseau the creation of a great legislator, a Numa or a Moses: they did not develop and perfect themselves in political

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*Political Writings*, edited Vaughan, op. cit., Vol. I pp. 314–318; Shklar, J. N., "Rousseau's Two Models: Sparta and the Age of Gold," *Political Science Quarterly*, Vol. LXXXI (March 1966), 25–51.

[11] Rousseau, J.-J., *Le Bonheur Public*, in *Political Writings*, edited Vaughan, op. cit., vol. I pp. 325–326.

[12] Rousseau, J.-J., *Le Contrat Social*, translated F. Watkins, in *Political Writings*, (Thomas Nelson and Sons, Edinburgh, 1953), p. 42 (hereafter cited as CS).

[13] Rousseau, *Le Bonheur Public*, op. cit., p. 326.

[14] *Ibid.*

[15] Rousseau, CS, op. cit., p. 58.

[16] *Ibid.*

[17] *Ibid.*, p. 42.

[18] Rousseau, J.-J., *Gouvernement de Pologne*, translated F. Watkins, in *Political Writings*, op. cit., pp. 166–167 (hereafter cited as *Pologne*).

[19] Rousseau, J.-J., *Économie Politique*, translated G. D. H. Cole, (E. P. Dutton and Co., New York, 1950), p. 308 (hereafter cited as *Éc. Pol.*).

[20] *Ibid.*

[21] Rousseau, J.-J., *Émile* (excerpt), in *Political Writings*, edited Vaughan, vol. II p. 145.

experience, but were "handed down" by the law-giver.[22]

But if Rousseau thought highly unified ancient polity, and its political morality of common good, superior to modern fragmented politics, and its political morality of self-interest, at the same time he shared with modern individualist thought the conviction that all political life was conventional and could be made obligatory only through individual consent. He very definitely thought that he had based political obligation and rightful political authority on convention: "civil association is the most voluntary act in the world; since every individual is born free and his own master, no one is able, on any pretext whatsoever, to subject him without his consent."[23] Indeed, the first four chapters of the *Contrat Social* are devoted to refutations of erroneous theories of obligation and right (paternal authority, the "right of the strongest," and obligation derived from slavery). "Since no man," Rousseau concluded, "has natural authority over his fellow men, and since might in no sense makes right, convention remains as the basis of all legitimate authority among men."[24]

And yet Rousseau was also clear that contract theory provides, in itself, little more than a mere theory of political obligation. In the *Lettres de la Montagne* (VI), speaking of contract and consent, he said that a state is made one by the union of its members; that this union is the consequence of obligation; and that obligation can follow only from convention. He admitted that the foundation of obligation had divided political theorists: "according to some, it is force; according to others, paternal authority; according to others, the will of God."[25] All theorists, he said, establish their own principle of obligation and attack that of others. "I myself have not done otherwise," Rousseau declared, "and, following the soundest element of those who have discussed these matters, I have settled on, as the foundation of the body politic, the contract of its members." And he concluded by asking, "what more certain foundation can obligation among men have, than the free agreement of him who obliges himself?"[26]

One may suspect that for Rousseau, contract theory was more a way of destroying wrong theories of obligation and authority, than of creating a comprehensive theory of what is politically right. While for Hobbes and Locke the theory of obligation by consent is of central importance, for Rousseau it is not a complete political theory. Any political system "which confines itself to mere obedience will find difficulty in getting itself obeyed. If it is good to know how to deal with men as they are, it is much better to make them what there is need that they should be" (*Économie Politique*).[27] That, in a word, was Rousseau's criticism of all contract theory: it dealt too much with the form of obligation, with will as it is, and not enough with what one ought to be obligated *to,* and with will as it might be.

His criticism of Hobbes is based on this point. Hobbes had, indeed, established rightful political authority on consent, rejecting paternal authority and obligation based on either natural or divine law; he had made law (and therefore morality) the command of an artificial "representative person" to whom subjects were "formerly obliged" through transfer of natural rights (save self-defense) by consent.[28] But Hobbes had done nothing to cure the essential wrongness (in Rousseau's view) of modern politics; private interest was rampant, and indeed paramount, in Hobbes' system (could one not decide whether or not to risk one's life for the Hobbesian state?). The essential error of Hobbes, Rousseau thought, was to have read back into the state of nature all the human vices which half-socialization had created, and thus to see culturally-produced depravities as "natural" and Hobbesian absolutism, rather than the creation of a feeling of a common good, as the remedy for these depravities. "The error of Hobbes and of the philosophers," Rousseau declared, "is to confound natural man with the men they have before their eyes, and to carry into one system a being who can subsist only in another" (*L'État de Guerre*).[29] Rousseau, who thought that a perfectly socialized state (like Sparta) could elevate men, and turn them from "stupid and limited animals" into moral and intelligent beings,[30] must have thought Hobbesian politics incomplete, one "which confines itself to mere obedience," one which did not attempt to make men "what there is need that they should be," but which, through a system of mere mutual forbearance,[31] did not undertake any improvement in political life. "Let it be asked," said Rousseau, "why morality is corrupted in proportion as minds are enlightened";[32] Hobbes might well have an enlightened view of obligation (to the extent that he based it on consent), but he did nothing about the moral corruption caused by "private interest" and "individual will."

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[22] Rousseau, *Pologne,* pp. 163–165.

[23] Rousseau, CS, p. 117.

[24] *Ibid.,* p. 8.

[25] Rousseau, J.-J. *Lettres de la Montagne,* in *Political Writings,* edited Vaughan, vol. II pp. 199–200.

[26] *Ibid.*

[27] Rousseau, *Éc. Pol.,* p. 297.

[28] Oakeshott, Michael, *Rationalism in Politics,* (Methuen and Co., London, 1962), pp. 258 ff.; Oakshott, Michael, Introduction to Hobbes' *Leviathan,* (Basil Blackwell, Oxford, 1957), pp. XXXV–L.

[29] Rousseau, J.-J., *L'État de Guerre,* in *Political Writings,* edited Vaughan, vol. I p. 306.

[30] Rousseau, CS, p. 20.

Rousseau, then, held in his mind, at once, both the idea that the closely unified political systems of antiquity (as he idealized them) were the most perfect kinds of polity, and the notion that all political society is the conventional creation of individual wills through a social contract. Holding both these ideas created problems, for while the need for consent to fundamental principles of political society, for creation of a mere political construct through "will and artifice," are doctrines characteristic of what Michael Oakeshott has called the "idiom of individuality,"[33] the ancient conception of a highly unified and collective politics was dependent not only on a morality of the common good quite foreign to any insistence on individual will as the creator of society and as the basis of obligation (and Rousseau sometimes recognized this, particularly in the *Économie Politique*), but was also dependent on a view of political life as the highest, most all-embracing end of man, and was, moreover, considered both natural, and prior, ontologically if not chronologically, to the independent existence of self-sufficient men. Given the ancient view of the morality of the common good and the supreme importance and naturalness of political life, ancient thought had not had to create theories of political obligation (which are needed only when the duty to obey is in doubt); politics being the highest end of man, obligation was not a real problem, and the task of the great legislator was not to show why men ought to obey, but merely in what way—under which kind of regime—they should do so. Legislation was the task of giving the most perfect form to an intrinsically valuable activity.

Rousseau, not being a systematic philosopher (as he often pointed out), never really reconciled the tensions between his theory of obligation, and his model of political perfection. If one wants to make Rousseau more consistent than he cared to be, one must admit that his ancient ideal model, as the creation, not of a contractual relation of individual wills, but of a great legislator working with political education and a common good morality, is not "obligatory" on citizens, is not founded in right. It is true that Rousseau sometimes spoke as though ancient systems were constructed by mutual individual consent; but he did not usually speak in those terms. Even though, for him, all political society, ancient or modern, is artificial in the sense that it is not the original condition of man, contract theory comprehends an additional element of artifice, namely the notion that a society must be created by the will of all its members. Rousseau rarely spoke as though ancient polity had been artificial in this sense; he usually said that ancient systems were created, not by contract, but by the genius of legislators like Moses and Lycurgus. Moses, for example, "had the audacity to create a body politic" out of "a swarm of wretched fugitives"; he "gave them customs and usages."[34] Lycurgus "undertook to give institutions" to Sparta; he "imposed on them an iron yoke" (*Gouvernement de Pologne*).[35] It is, really, only in the *Contrat Social* that Rousseau makes much reference to consent or contract in ancient politics; the usual emphasis (as in the *Économie Politique* and *Gouvernement de Pologne*) is on great men, political education, and the absence of a highly developed individual will.[36]

One can say, moreover, if one wants to juxtapose parts of Rousseau's thought to each other, that he made fundamental errors in analyzing the unified spirit of ancient politics, by recognizing the desirable *effects* of a morality of the common good, without recognizing that the very absence of a notion of individual will as supreme had made that morality (and thereby that unity) possible. But, after all these things have been pointed out, it remains to be said that Rousseau was consistently clear that modern calamities caused by self-interest must be avoided, and that the political systems created by ancient legislators were better than any modern ones. Although it did not always occur to Rousseau that both the merely self-interested will which he hated, and the will necessary for consent to conventional society, were part of the same idiom of modern political thought, and perhaps inseparable, he always thought that mere will, as such, could never create a proper political society. For him, then (whatever the confusions over naturalness, will, or the presence or absence of either or both in any political idiom), the problem of political theory, above all in the *Contrat Social,* became that of reconciling the requirements of consent (which obligates) and perfect socialization (which makes men "one"); men must somehow choose the politically perfect, somehow will that complete socialization which will preclude self-division. Will, though the basis of consent, cannot be left as it is in traditional contract theory, with no proper object. If it is true that it is the source of obligation, it is also true that merely self-interested will is the cause of everything Rousseau hated in modern civilization.[37] And perfect political forms, Whatever Rousseau might have said about their being "given," must now (in the *Contrat Social*) be willed.

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[31] Oakeshott, *Rationalism in Politics,* p. 261.

[32] Rousseau, *L'État de Guerre,* p. 307.

[33] Oakeshott, *Rationalism in Politics,* pp. 249–251.

[34] Rousseau, *Pologne,* pp. 163–165.

[35] *Ibid.*

[36] Rousseau, *Éc. Pol.,* pp. 293–311.

Setting all the contradictions and vacillations aside, then, there are two important elements in the two views that Rousseau held simultaneously: first, that the importance of ancient polity had to do with its unity and its common morality, and not with its relation, or lack of it, to contract theory; second, that individual consent (whatever this might do to the "legitimacy" of Sparta) is needed for obligation, which is needed because the state is conventional. It is impossible to make every element of Rousseau both consistent and true to the political principle that he tried to establish: that will is not enough, that perfect polity alone is not enough, that will must be united to perfection, and that perfection must be the standard of what is willed. And this, perhaps, is the source of that odd idea, *general will:* a fusion of the generality (unity, communality) of antiquity with the will (consent, contract) of modernity. What makes Rousseau, without doubt, the most utopian of all great political theorists, is his insistence that even a perfect political system be willed by all subject to it. "Undoubtedly," he said, "there is a universal justice derived from reason alone; but this justice, to be admitted among us, must be mutual . . . conventions and laws are necessary, therefore, to unite rights with duties, and to accomplish the purposes of justice."[38] Though "that which is good and conformable to order is such by the nature of things, independent of human conventions,"[39] those conventions are yet required.

Rousseau's political thought is a noble attempt to unite the best elements of contract theory, of individual consent, with his perfect, unified ancient models, which, being founded on a morality of common good, had no private wills to "reconcile" to the common interest (which was natural), and thus no need of consent, no need of contract. It is this (perhaps unconscious and certainly unsystematic) attempt to fuse two modes of political thought, to have common good *and* individual will, which gives Rousseau's political thought that strange cast which some have thought contradictory, a vacillation between "individualism" and "collectivism," but which was not merely that. The problem for Rousseau was more specific and more subtle: how one can obey only his own free will, the source of obligation, in society; how it is possible to purify this will of mere private interest and selfishness, which create inequality, destroy virtue, and divide man against himself; how it is possible to insure that this individual will will want only what the common good—preferably a common good like that of ancient politics—requires. It is really a problem of retaining will, but of making it more than mere will, so that society will have a common good and a general interest, *as if it enjoyed a morality of the common good*—a morality which Rousseau sometimes recognized as the real foundation of ancient unity.

Looked at from this point of view, all of the paradoxes and "problems" in Rousseau's theory become comprehensible: why will must be retained, and why it must be made "general"; how general laws will promote the common good, but why not law, but legislative will, is final; why a great legislator can suggest perfect political forms, and why he cannot merely impose them. Above all, it is clear that this point of view helps to explain the greatest paradox in all of Rousseau—that is, the paradox created by the fact that, in the original contractual situation, the motives needed by individuals to relinquish particular will and self-interest, and to embrace a "general" will and the common good, cannot exist at the time the compact is made, and can only be the *result* of the socialization and common morality that society alone can create.[40] It is certain that if either an ideal of social perfection (Sparta), or a notion of conventional society created by mere will, were enough for Rousseau, he never would have insisted on a combination of will and perfect socialization, on a general will. There would, in fact, be no paradox at all, if perfection were only a formal question, if the state were founded on a morality of the common good and obligation were not a central problem. A great legislator, like Moses or Lycurgus, could create the best forms, and obedience would be only a question of correspondence to a system naturally and rationally right. But Rousseau said that a new-born people must, in order to will good laws, be able to "appreciate sound political principles";[41] these cannot be merely given to them, but must be willed. Why is it that "the social consciousness to be created by the new institutions would have to preside over the establishment of those same institutions,"[42] unless, somehow, the people must understand and will the system? There would be no paradox of cause and effect (the central problem of sound politics) in Rousseau if men did not have both to will, and to will a perfection which presupposes (on Rousseau's own terms) a transcendence of mere will, and the attainment of all the advantages of a morality of the common good, without actually having that morality, which would destroy obligation.

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[37] *Ibid.*, pp. 307–309.

[38] Rousseau, CS, pp. 37–38.

[39] *Ibid*, p. 37.

[40] *Ibid.*, p. 44. Cf. Rousseau, J.-J., *Des Lois*, in *Political Writings*, edited Vaughan, vol. I p. 331.

It remains to show that the attempted fusion of individual will and common-good morality is comprehended in the notion of the general will.

## II

Rousseau begins the *Contrat Social,* not with the conception of general will, but with a fairly traditional contractarian view of the origin of society. Men being naturally (if not by nature)[43] perfectly independent and society made necessary only by the introduction of property (and the consequences of this introduction), men unite by contract to preserve themselves and their property. In this conventional society, there is an area of common interest, "for if the opposition of private interests made the establishment of societies necessary, it is the agreement of these same interests that made it possible."[44] It is "what these several interests have in common," says Rousseau, "that constitutes the social bond." It is only on the "basis of this common interest that society must be governed."[45]

Rousseau does not talk in these rationalistic, contractarian terms for very long. Soon enough it turns out that society's "common interest" is not merely what a lot of private interests have in common: society, when it is perfect, is a complete transformation of these private interests; it is only when "each citizen is nothing, and can do nothing, without all the rest," that society "may be said to have reached the highest attainable peak of perfection."[46] It is only, in other words (keeping in mind which societies had attained peaks of perfection), when society is much like highly unified ancient society, that "perfection" is reached; it is only "in so far as several men conjoined consider themselves as a single body"[47] that a general will can operate.

This transformed society must be governed on the basis of common interest (which has become something more than traditional common "interest"); only general laws, the creation of a general will (sovereignty), can govern the common interest.[48] Laws must be perfectly general, because the general will which makes them "loses its natural rectitude when directed toward any individual and determinate object."[49] The sovereign (the people when "active," when willing fundamental law) must, of course, make such law: "the people subject to the laws should be their author; only those who are forming an association have the right to determine the conditions of that society."[50] But, if fundamental law is the creation of a general will, how does such a will come about? It cannot be the sum of individual wills, for "the particular will tends by its nature to partiality,"[51] and this, clearly, has been the source of modern "calamities." Law must be willed by those subject to it, for will is the source of obligation. Yet mere wills can never yield generality, and law must be perfectly general, which can happen only "when the whole people legislates for the whole people."[52] If general laws alone, composed with a view to the common good, were enough, there would be no problem; but even the most general laws must be willed. How can a self-interested multitude "by itself execute so great and difficult a project as a system of legislation?"[53] How can a genuine general will, which will create general "conditions" for society, arise?

It is on this point that Rousseau is weak, uncharacteristically weak; he is always able to say what a general will must *exclude,* but he cannot say what it *is.* And this should come as no surprise. For, strictly speaking, the idea of general will is an impossibility; the ideas of generality, and of will, are mutually exclusive. Will, whatever its crudity as a psychological construct, is characteristically a concept of individuality, of particularity, and it is only metaphorically that one can speak of will as "general." No act of philosophic imagination can conjure up anything but a personal will. What can be imagined (and what, in any case, Rousseau admired in ancient society), is not really a "general" will, but a political morality of common good, in which individual will is, to be sure, not suppressed, but simply does not appear in contradistinction to, or with claims or rights against, society. What gave ancient polity (particularly in theory) its unity was not the concurrence of many wills on central points of common interest; rather a moral idiom in which extreme socialization was natural, and in which there was little notion of "will and artifice," gave rise to this unity.

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[41] *Ibid.* (CS).

[42] *Ibid.*

[43] Since, for Rousseau, the pre-social man is a "stupid and limited animal," and becomes a moral and intelligent being only in society, there is a sense in which man's highest nature (rather than original nature) *is* social. Cf. Rousseau, J.-J., *Lêttre à M. Philopolis,* in *Political Writings,* edited by Vaughan, vol. I, p. 223.

[44] Rousseau, CS, p. 25.

[45] *Ibid.*

[46] *Ibid,* p. 42.

[47] *Ibid.,* p. 113.

[48] *Ibid.,* pp. 26, 29–30, 38–40.

[49] *Ibid.,* p. 32.

[50] *Ibid.,* p. 40.

[51] *Ibid.,* p. 25.

[52] *Ibid.,* p. 38.

[53] *Ibid.,* p. 40.

There are a number of revealing passages in which Rousseau observes that something like a political morality of common good, rather than a general will, is necessary for the unity and communality which he desires—for example, at the end of Book II of the *Contrat Social*, where he discusses "the most important of all" laws, "which preserves the people in the spirit of its original institutions," that is, "manners, morals, customs and, above all, public opinion . . . a factor with which the great legislator is secretly concerned when he seems to be thinking only of particular regulations."[54] Here Rousseau does not, of course, speak of consent or will, but of a kind of political education which will promote a common good.

And he gave other indications that he knew that unity was the consequence of thinking about political relations in terms of a common good, rather than in terms of "cancelling out" of private wills "all the mutually destructive pluses and minuses," so that a "general will remains as the sum of the differences."[55] Indeed, in the *Économie Politique* he said that if men "were early accustomed to regard their individuality only in its relation to the body of the state, and to be aware, so to speak, of their own existence merely as a part of that of the state, they might at length come to identify themselves in some degree with this greater whole. . . ."[56] If children are educated in state laws which promote only a common good and a common morality, "they will learn to cherish one another mutually as brothers, to will nothing contrary to the will of society."[57] There are similar passages in the *Gouvernement de Pologne*, and in the *Projet pour la Corse*, in which the idea of a morality of the common good, reinforced by political education and legislation, is set forth, with little or no reference to consent or to will.[58]

But, much of the time, and particularly in the *Contrat Social*, Rousseau speaks not in the idiom of the common good, but of a tension between particular will and general will, and of *reconciling* these wills.[59] Indeed, the whole concept of political virtue is entirely tied up with this reconciliation of wills, as Rousseau demonstrates in the *Économie Politique*. There he says that the "first and most important rule of legitimate or popular government" is "to follow in everything the general will," but that, in order to follow the general will it must be known, and clearly distinguished from individual or particular will, and that "this distinction is always very difficult to make, and only the most sublime virtue can afford sufficient illumination for it."[60] A few pages later, he says that "if you would have the general will accomplished, bring all particular wills into conformity with it; in other words, as virtue is nothing more than the conformity of the particular wills with the general will, establish the reign of virtue."[61] Is it not clear that the argument is circular?—that conformity of particular to general will creates virtue, and that virtue is necessary to bring particular will into conformity with general will? This circularity is not due to the fact that Rousseau had no clear conception of virtue; on the contrary, ancient politics were models of virtue, as he described them.[62] The circularity is caused by trying to make virtue *as* unity, *as* communality, dependent on reconciliation of particular to general will, whereas in fact (and as Rousseau recognizes on adjoining pages of the same treatise), virtue as conformity to a common good morality is the creation of great legislation and political education. Vacillation on the true source of ancient unity and communality (reconciliation of wills, or absence of will), is the cause of the circularity of the concept of virtue in Rousseau; moreover, this circularity reflects the same kind of cause-effect paradox as that referred to at the end of part I. In both cases, Rousseau knows perfectly well what he admires: the virtue of ancient society, and the perfection of laws in ancient society. It is only when, in both cases, he tries to describe the possibility of these attributes in terms of reconciled will that he falls down. Nor is this surprising; for, as has been noted, he did, sometimes, recognize that the absence of the very notion that caused all his problems—will as supreme—had constituted the greatness of antiquity. All the paradoxes, circularities and vacillations in Rousseau are caused by the attempted fusion of moral-political idioms which are incompatible on fundamental points.

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[54] *Ibid.*, p. 58.

[55] *Ibid.*, p. 29.

[56] Rousseau, *Éc. Pol.*, pp. 307–308.

[57] *Ibid.*, p. 309.

[58] Rousseau, *Pologne*, chapters II-IV; Rousseau, J.-J. *Projet pour la Corse*, translated F. Watkins, in *Political Writings*, p. 300.

[59] Rousseau, CS, pp. 40–41.

[60] Rousseau, *Éc. Pol.*, p. 293.

[61] *Ibid.*, p. 298.

[62] Rousseau, *Pologne*, chapters II–III; Rousseau, *Rome et Sparte*, in *Political Writings*, edited Vaughan, vol. I pp. 314–320.

A clear illustration of just this point is Rousseau's treatment of Brutus in the *Histoire des Moeurs;* in that fragment Rousseau, observing that "it will always be great and difficult to submit the dearest affections of nature to country and virtue."[63] cites Brutus' execution of his treasonous sons as an example of this "submission," not ever mentioning that this was no case of "submitting" particular will to a general will, but (more likely) a case of a common-good morality (coupled with the rights of the Roman *pater familias*) at work. Yet in other works written at about the same time, he makes no reference to submission of will in Roman society, and talks only of legislation and political education.[64]

Nonetheless (and despite these vacillations), it is easy to see why a fusion of political idioms was attempted. For even though perfect socialization was Rousseau's ultimate ideal, consent and will, as the source of obligation, were too important to be summarily discarded. Thus the general will, though an impossibility, was a necessity.

"Actually," said Rousseau, "each individual may, as a man, have a private will contrary to, or divergent from, the general will he has as a citizen" (*Contrat Social*).[65] This could not, of course, be the case in a state with a common-good morality reinforced by legislation and education (the system sketched in the *Économie Politique*). The passage cited from the *Contrat Social* shows that in that work, the most "contractarian" of Rousseau's writings, and the closest thing he had to a systematic political theory, neither mere will nor perfection wins out alone. For in the *Contrat Social,* there is the possibility that a private person (already a concept of modern individualism) may regard "the artificial person of the state as a fictitious being," and that this "may make him envisage his debt to the common cause as a gratuitous contribution."[66] It seems clear that if Rousseau were *not* trying (however unsystematically) to reconcile will and perfect socialization, these problems could not exist: the new state could not be considered a fictitious being (for it would educate men to think something different); one would not think of his political role as a "contribution" (because one would be naturally part of a greater whole); and there would be no conflict between man and citizen (because the distinction would not exist). Is not the paradox that a man must be "forced to be free"[67] if his particular will does not conform to the general will an indication that Rousseau tried to gain the advantages of a common-good morality (particularly in the *Contrat Social*) through reconciliation of wills, and this only because will is necessary to obligation?

There is, finally, in Rousseau's most systematic political work (the *Contrat Social*), no postulation of a political morality of the common good as the source of the much-desired unity. Rather, there is a constant attempt to bring particular will into conformity with general will through the efforts of a "great legislator." What the great legislator, in his wisdom, knows to be good, supplies the absence of a common-good morality. Now, the difference between the great legislator of ancient politics and Rousseau's ideal legislator, corresponds exactly to the difference between giving a presupposed unity (without strong will) a perfect form (antiquity), and making people *will* perfect forms (modernity). "All" in the contractual period "stand equally in need of guidance."[68] Individuals "must be obliged to bring their wills into conformity with their reason," that is, they must will that which is, in itself, rationally best. The combination of individual consent and the legislator's guidance "will effect a union of understanding and will within the social body."[69] And what is rationally best (to avoid that self-division caused by half-socialization) is the perfectly united and communal polity of antiquity. The legislator, who effects the bringing of will into conformity with reason (not by force, but through persuasion and religious devices), supplies the defect of common-good morality, and simply gets each individual to will something like the laws which *would have* resulted from such a morality. What Rousseau ultimately has, then, is not a "general will" (which is inconceivable), but a "will to the general," which is conceivable if one recalls that political perfection requires both truly general laws and consent to them. Rousseau did not, could not, abolish will—but he prescribed the form that it must take, and this form is clearly derived from the generality and unity of ancient polity (as Rousseau saw it)—but without a morality of common good, which would have destroyed obligation.

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[63] Rousseau, J.-J., *Histoire des Moeurs,* in *Political Writings,* edited Vaughan, vol. I p. 337.

[64] Rousseau, *Éc. Pol.*, pp. 302–310.

[65] Rousseau, CS, pp. 18–19.

[66] *Ibid.*, p. 19.

[67] *Ibid.*

[68] *Ibid.*, p. 40.

[69] *Ibid.*

Moreover, not only the form of laws (generality) is derived from ancient models; the *conditions* under which good laws (and indeed good states) are possible, are little more than idealizations of ancient political circumstances. A people is "fit for legislation," according to Rousseau, if it has no old laws, if it is free from threats of invasion and can resist its neighbors, if it is small enough that its "individual members can all know one another," if it can get along without other peoples, if it is "neither very rich nor very poor," and can be self-sufficient, and if it is one which "combines the firmness of an old people with the docility of a new one."[70] Clearly most of these conditions of the possibility for good polity are abstracted from Rousseau's idealized version of ancient city-states, particularly Sparta. Not only the form of a good political system, then, but also the empirical conditions which could make such a system possible, are derived from Rousseau's models of perfection.

What has been said thus far may be summarized in this way: 1) a perfect state (that is, a perfectly socialized, united and communal state) would have perfectly general laws (that is, laws dealing only with Rousseau's vision of a common good); 2) but laws, especially the most general laws, must be willed by everyone subject to those laws, in order to be obligatory—and they must be made obligatory, for society is merely conventional; 3) therefore, will must take the form of general laws; 4) but will tends to the particular, and law, though the creation of will, must somehow be general; 5) moreover, for particular wills to appreciate the necessity of general laws, effect would have to become cause; 6) therefore, a great legislator, whose instruction can supply the defect of a morality of the common good—the only morality which would *naturally* produce general laws—is necessary; 7) but this legislator is impossibly rare, and, in addition, he cannot create laws, however general and good, for sovereignty is inalienable; 8) thus the legislator must have recourse to religion, and use it to gain the consent of individuals to the general will; 9) but now "consent" is something less than real consent, since an irrational device has been used; 10) finally, the whole system is saved for individual will by the fact that "a people always has a right to change its laws, even the best,"[71] that legislative *will*, rather than law itself, is supreme, and that the entire social system can be abolished by will, for "there is not, and cannot be, any sort of fundamental law binding on the body of the people, not even the social contract."[72]

The theory of Rousseau, then, ultimately reduces itself to two elements: the need for a great legislator to create a "general will," and the extreme limitations put on this legislator by the fact that this general will, though (of course) general, is still will, and must be will. Both elements are the consequence of attempting to unite all the requirements of will with all the advantages of perfect socialization. The legislator may formulate and propose general laws which will produce perfect socialization, and he can get them "willed" through religious devices; but the sovereign cannot be permanently bound, even by perfect laws, and can change these laws and even dissolve society. Thus neither perfection nor will has all the claims in Rousseau; but will can finally be, even if only in a destructive way, triumphant, for if a people "is pleased to do itself harm, who has a right to prevent it from doing so?"[73]

It is not meant, by analyzing Rousseau in this way, that he was always perfectly consistent in desiring that particular wills should consent to that which an ancient morality of common good would require; indeed, he vacillated on several points, notably in his treatment of civil religion, in which he allowed any tolerant sect to exist, so long as it did not claim exclusive truth or refuse to subscribe to the basic articles of civic religious policy.[74] Above all, though, Rousseau insisted that each socialized man should somehow "obey no one but himself," and thought he had found a solution to this problem in making the conditions of society (laws) perfectly general and equally applicable to all, so that, the conditions being equal for all, and willed by all, "it is in the interest of no one to make [social requirements] burdensome to the rest,"[75] and that, since the society cannot wish to hurt all its members by enacting bad general laws, society need offer its members no guarantees.[76] But this system is essentially modified by the fact that a will to general laws absolutely cannot be attained with mere wills as they are (the cause-effect problem again), but only through the influence of a great legislator; by Rousseau's assertion that there is no real limit to the extent of undertakings possible between the sovereign and its members;[77] by the idea that the sovereign is the sole judge of how many "powers" of individuals must be socialized;[78] and by the notion that an ideal society should be very highly socialized indeed. It is modified above all by part IV, chapter II of the *Contrat Social,* which shows, perhaps more clearly than anything in Rousseau, that consent is no longer to be a question of mere volition, and that the general will is something like a modified common-good morality.

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[70] *Ibid.*, pp. 53–54.

[71] *Ibid.*, p. 57.

[72] *Ibid.*, pp. 17–18.

[73] *Ibid.*, p. 57.

[74] *Ibid.*, pp. 152–155.

[75] *Ibid.*, p. 15.

[76] *Ibid.*, p. 18.

> The constant will of all the members of the state is the general will; that is what makes them citizens and free. When a law is proposed in the assembly of the people, what the voters are being asked is not precisely whether they do or do not approve of the proposal, but whether or not it is in conformity with the general will, which is their own. Each, when casting his vote, gives his opinion on this question; and the declaration of the general will is found by counting the ballots. Thus when an opinion contrary to my own prevails, this proves nothing more than that I was mistaken, and that what I though to be the general will was not.[79]

The meaning of this (usually confusing) passage can be understood if "common good" be substituted for "general will"; then it can be seen how general will is *constant* will, and how citizens are not being asked whether they approve a proposal, but whether it is in conformity with a common good.

With all of these modifications in mind, it is clear that, while Rousseau's theory of society and law really is, as he insisted, an attempt to preserve liberty, that liberty is conceived in an odd way: it is "obedience to self-imposed law"[80] (which must, of course, be general law). Liberty, then, comes down to freeing the individual of all private dependence by making him "very dependent on the city" and its general and equally-applied laws. But, though liberty is obedience to self-imposed law, proper law cannot be created without modification of will by a great legislator, and thus the very idea of liberty is, like other elements of Rousseau, a fusion of the idioms of individual will and of highly unified society. It is because of these modifications that Rousseau's political theory cannot be so easily assimilated to traditional constitutionalism, or to Kant's theory of law, as some have imagined.[81] For legislative will, and not law itself, is supreme in Rousseau. Nor can Rousseau be easily assimilated to the German "romantic" tradition of the early 19th century, because he would never have replaced general *will* with the historical evolution of national *spirits;* he was clear that history, in itself, cannot "justify" anything.[82] This non-assimilability to other traditions proves that those who view Rousseau as a unique and isolated figure are probably correct; he was, in his own words, one of those few "moderns who had an ancient soul."[83]

## III

The only object of this study has been to elucidate the concept of the general will, and to clear up some of the paradoxes in the *Contrat Social,* by analyzing what Rousseau thought about will and contract, on the one hand, and about perfect political systems, on the other. It has not been a central object to attack Rousseau as unsystematic, to reproach him for not adequately reconciling two modes of political thought, or to "improve" his ideas by making them more consistent. No series of conceptual ambiguities can detract anything from Rousseau's status as the greatest of political psychologists and the most eloquent critic of the psychological destruction wrought by inequality. Nonetheless, it is evident that there is an insurmountable conceptual problem in Rousseau's political thought, and it is this: voluntarist theories are usually composed of two parts, a theory of will as a moral agency, and a theoretical standard of right to which will ought to conform (arbitrary willfulness usually being rejected as a standard of right). In Locke or in Kant, the standard of right to which will must conform (natural law in the first case, universal rational law in the second) does not contradict voluntarism (once one gets around the problem of reconciling free will and absolute standards: in itself a problem which defeated a Leibniz and reduced Kant to hypotheses). But in Rousseau the standard to which will must conform (ancient perfection, or its equivalent) is itself non-voluntaristic; and therefore will and the standard to which it must conform are contradictory. The standard which gives will its object is the very negation of voluntarism. And it is for this reason that Rousseau's political system is somewhat paradoxical. The idea of general will, the paradox of cause and effect in the contractual situation, the circularity of the concept of virtue—all these are due to an attempt to fuse the advantages of a politics founded on will, and of one founded on reason and perfection. One must, in Rousseau, will the kind of society in which one lives, because "to deprive your will of all freedom is to deprive your actions of all morality,"[84] and this deprivation destroys the obligation one has to obey. Yet mere will can yield only particularity and inequality, and will never produce rational perfection. To retain the moral attributes of will, but to do away with will's particularity and selfishness—that is the problem of Rousseau's political thought. It is a problem which reflects the difficulty which Rousseau found in making free-will and rational authority co-exist in his moral and political thinking. Freedom of the will is as important to the morality of actions for Rousseau as for any traditional voluntarist; but he was suspicious of the very faculty, the only faculty, which could moralize. And that is why he urged, in the *Économie Politique,* that "the most absolute authority is that which penetrates into a man's inmost being, and concerns itself no less with his will than with his actions."[85] Can the will be both morally autonomous and subject to the rationalizing influence of authority? That was the point which Rousseau never settled altogether. Even Émile, the best-educated of men, chooses to continue to accept the guidance of his teacher.[86] How much more, then, do ordinary men need the guidance of a great legislator when they embark on the setting up of a system which will not only aid and defend, but also *moralize* them! The relation of volition to authority is one of the most difficult and one of the most inscrutable problems in Rousseau; the general will is dependent on " a union of will and understanding within the social body,"[87] but that understanding, which is provided by authority, weakens the idea of will as an autonomous authorizing faculty.

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[77] *Ibid.,* p. 33.

[78] *Ibid.,* p. 31.

[79] *Ibid.,* pp. 117–118.

[80] *Ibid.,* p. 20.

[81] Cassirer, Ernst, *Rousseau, Kant and Goethe,* translated Gutmann, Kristeller and Randall, (Harper Torchbacks, New York, 1963), pp. 30 ff.

[82] Rousseau, J.-J., *Première Version du Contrat Social,* in *Political Writings,* edited Vaughan, vol. I p. 462.

[83] Rousseau, J.-J., *Jugement sur la Polysynodie,* in *Political Writings,* edited Vaughan, vol. I p. 421.

To project the question on a grander scale, one can see in Rousseau's political thought an intuitive attempt to reconcile the two greatest traditions of Western political philosophy, that of "will and artifice" and that of "reason and nature." (Oakeshott) For general will is surely rationalized will. And yet it is not self-rationalized will in a Kantian sense, but will rationalized by the standards and conditions of idealized ancient polity.

Whatever Rousseau's means, in undertaking a fusion of two great modes of political thinking, and however unsuccessful the attempt to make general will a viable conception, one must always, while analyzing and even criticizing the result, grant the grandeur and importance of the effort. For if one could succeed in having the best of both idioms, one would have a political philosophy which would synthesize almost everything valued in the history of Western political thought.

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[84] Rousseau, CS, p. 9.

[85] Rousseau, *Éc. Pol.,* p. 297.

[86] Rousseau, J.-J., *Emile,* translated B. Foxley, (Everyman's Library, J. M. Dent and Sons, London 1911), p. 444.

[87] Rousseau, CS, p. 41.

# INTERNATIONAL SUBSYSTEMS: STABILITY AND POLARITY

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## I. BIPOLAR OR MULTIPOLAR STABILITY?

One of the current controversies within international relations deals with the "stability" of bipolar as opposed to multipolar stratifications of world power. Morton Kaplan, in codifying the views of classical balance of power theorists,[1] advances the view that multipolar systems are more stable than bipolar systems.[2] Kenneth Waltz, sagely pointing to the relatively peaceful international arena since World War II, argues that a bipolar distribution of power can guarantee world stability.[3] Many a priori arguments have been presented to buttress the Kaplan and Waltz hypotheses.[4] In one of the most elaborate such formulations, the "interaction opportunity" hypothesis of Karl Deutsch and J. David Singer, the presence of stabilizing cross-cutting alliances is postulated to be most likely within multipolar systems, which in turn are a function of the number of major powers and members of a system.[5] In an attempt to bring the two opposing strands of theory into a larger framework, Richard Rosecrance more recently has suggested that bipolarity and multipolarity may each have their peculiar costs and benefits.[6] Bipolarity, according to Rosecrance, is distinguished by (1) an absence of "peripheries," such as areas for colonial expansion or neutral powers to woo; (2) all international behavior is highly politicized; (3) there are many crises; (4) changes in power confrontations are either significant or trivial, with no intervening shades of gray; (5) each pole is dominated by major powers highly motivated to expand their domains, willing even to incur brinksmanlike situations and hostility spirals; (6) no detente is possible. Multipolarity, on the other hand, is hypothesized to have (1) more interaction opportunities and thus less preoccupation (or obsession) with any one set of states; (2) fewer arms races; (3) more international conflicts; (4) the outcomes of international conflicts are harder to predict in advance; (5) changes in power confrontations have ambiguous consequences for the overall distribution of power. Rosecrance, therefore, urges a "bi-multipolar" arrangement that would combine the best features of both alternatives. The empirical questions and intriguing hypotheses so eloquently raised by Kaplan, Waltz, Deutsch, Singer, and Rosecrance have remained largely unexamined, however.

To date only two efforts have been made to assemble data for a test of the proposition that international stability is related to the stratification of power in international systems. In one study, Rosecrance establishes nine historical "international systems" between 1740 and 1960 in terms of differing power configurations.[7] After delineating a number of qualitative categories for describing an international system, he supplies codings for each of the nine systems and proceeds to conduct a necessary and sufficient condition analysis of preconditions to *stability*, which he defines as the ability of a regulator to cope with disturbances threatening to undermine an existing power distribution (where the ratio of regulator strength/level of disturbance is high). With respect to power distribution, the lone unipolar system in his sample is fairly stable, but multipolarity is as Kaplan predicts clearly more stable than bipolarity. Considering "resources" either as areas ripe for colonial and imperial expansion or as new states that may be targets for ideological or political proselytization, Rosecrance's codings reveal that the availability of unappropriated resources makes for stability; scarcity, for instability. The existence of contending or non-status quo ideologies means that conflict will be intense, but a divergence in ethos between elites is neither a necessary nor a sufficient condition for instability, thus confirming Waltz' suspicion concerning the insignificance of ideological aspects of the contemporary bipolar world. Insecurity of domestic elite tenure at home is always associated with instability abroad. But the drawing of boundaries such that allegiances are nationalist rather than dynastic has not resulted in a more benign international arena; in fact, unstable systems have been encountered more frequently since the rise of nationalism and the use of citizen forces. The increasing world level in economic development is associated with a higher level of disturbance and fails to be channelled to augment the power of regulators of international systems. And no single regulator, whether balance of power, concert of great powers, alliance system, or international organization, has been able to maintain stability in all of the cases: the record of the concert is better, but the breakdowns of 1848 and 1914 were too extraordinary for an informal practice of summit conferences to avert or to resolve. Whenever the disturbance level is high, regulators have proved inadequate.

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[1] For statements of classical "balance of power" theory, see R. W. Mowat, *The European States System* (London: Oxford University Press, 1929); Carl J. Friedrich, *Foreign Policy in the Making* (New York: Norton, 1938); Hans J. Morgenthau, *Politics Among Nations* (4th ed.; New York: Knopf, 1967), Part IV; Frederick L. Schuman, *International Politics* (6th ed.; New York: McGraw-Hill, 1958), pp. 70–72, 275–78, 577–79, 591–92; Martin Wight, *Power Politics* (London: Royal Institute of International Affairs, 1946). See also Ernst B. Haas, "The Balance of Power: Prescription, Concept or Propaganda?" *World Politics,* V (July, 1953), 442–77.

[2] Morton A. Kaplan, *System and Process in International Politics* (New York: Wiley, 1957), Ch. II, V, VI; Kaplan, "Intervention in Internal War: Some Systemic Sources," *International Aspects of Civil Strife,* ed. James N. Rosenau (Princeton: Princeton University Press, 1964), pp. 92–121. For other codifications, see Inis L. Claude, Jr., *Power and International Relations* (New York: Knopf, 1962), pp. 11–93; Edward V. Gulick, *Europe's Classical Balance of Power* (Ithaca: Cornell University Press, 1955); George Liska, *International Equilibrium* (Cambridge: Harvard University Press, 1957), pp. 187–202; Arnold Wolfers, *Discord and Collaboration* (Baltimore: Johns Hopkins Press, 1962), pp. 117–32.

[3] Kenneth N. Waltz, "Stability of the Bipolar World," *Daedelus,* XCIII (Summer, 1964), 881–909.

[4] Among the advocates of multipolarity we may count Roger D. Masters, "A Multi-Bloc Model of the International System," this REVIEW, LV (December, 1961), 780–98. Bipolarity is favored by Raymond Aron, "The Quest for a Philosophy of Foreign Affairs," *Contemporary Theory in International Relations,* ed. Stanley Hoffmann (Englewood Cliffs: Prentice-Hall, 1960), pp. 79–91; Amitai Etzioni, *Political Unification* (New York: Holt, Rinehart, Winston, 1965), pp. 69–70. See, however, William E. Griffith, "Eppuor Se Muove," *Daedelus,* XCIII (Summer, 1964), 916–19.

[5] Karl W. Deutsch and J. David Singer, "Multipolar Power Systems and International Stability," *World Politics,* XVI (April, 1964), 390–406.

[6] Richard N. Rosecrance, "Bipolarity, Multipolarity, and the Future," *Journal of Conflict Resolution,* X (September, 1966), 314–27.

In a second study, J. David Singer and Melvin Small pay closer attention to alliance patterns. Starting with the hypothesis that the onset of war is associated with an increase in alliance commitments between countries, they assemble an impressive array of data on wars and alliances in order to test balance of power theory empirically.[8] For the years between 1815 and 1945 they collect statistical indicators for each member of the family of nations, and slice international systems in a manner not dissimilar to Rosecrance, for they analyze four subsamples of international systems—a "central system" composed of mostly European countries for two eras, 1815–1899 and 1900–1945, and a "total system" of central and peripheral countries for the same two eras. Although intercorrelations between number of alliances and number of wars are mostly positive across all years for the total and central systems, when results for the nineteenth century are compared with twentieth century findings, the two eras exhibit opposite patterns. For the 1900–1945 period most correlations are positive, demonstrating that modern wars have been preceded by the formation of many alliances; between 1815 and 1899, in contrast, correlations are mainly negative, revealing that alliance involvement may have served as a deterrent to the expression of international conflict by military means in the nineteenth century.

The two studies, however, were not originally designed to permit a comprehensive, systematic test of the relationship between power distribution and international stability. If we define a *pole* as a militarily significant cluster of units within an international arena, and *stability*[9] in terms of rates of incidence of warfare within the arena, we soon discover that the analytical procedures followed by Rosecrance, Singer and Small need to be clarified and tightened considerably, especially with respect to the criteria for selecting "international systems." One question to pose is *geographic*—whether Rosecrance's nine and Singer's and Small's four systems are international or in actuality are regional in scope. Rosecrance makes occasional references to events taking place in North America and Japan, but most of his attention is directed to European interstate politics; Singer and Small explicitly acknowledge that their "central" systems are European in composition. A second problem is the *dating* of separate eras. Rosecrance finds nine eras between 1740 and 1960, while Singer and Small arbitrarily divide the data into two sets, 1815–1899 and 1900–1945. Further discussion of these analytical problems seems warranted before undertaking a comparative test of the relationship between stability and polarity.

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[7] Richard N. Rosecrance, *Action and Reaction in World Politics* (Boston: Little, Brown, 1963). For some critiques, see George Liska, "Continuity and Change in International Systems," *World Politics,* XVI (October, 1963), 118–36; James N. Rosenau, "The Functioning of International Systems," *Background,* VII (November, 1963), 111–18; Dina A. Zinnes, "The Requisites for International Stability," *Journal of Conflict Resolution,* VIII (September, 1964), 301–305; J. David Singer, "The Global System and Its Sub-Systems: A Developmental View," *Linkage Politics,* ed. James N. Rosenau (New York: Free Press, 1969), pp. 21–43.

[8] J. David Singer and Melvin Small, "Alliance Aggregation and the Onset of War, 1815–1945," *Quantitative International Politics,* ed. Singer (New York: Free Press, 1968), pp. 247–86.

[9] "Stability" is most customarily defined as behavioral change over time, such as in Michael Haas, "Types of Asymmetry Within Social and Political Systems," *General Systems,* XII (1967), 69–79. Rosecrance's definition deals with the extent of regulatory potency in coping with instability.

Accordingly, the design of the research presented below will consist of an interweaving of analytical and empirical operations. Initially, the concept of "international subsystem" will be defined in such a way that analytical criteria can be used to detect subsystem arenas and members therein. For each of these entities, or units of analysis, it will be necessary to assemble indicators of the independent variable—stratification—and the dependent variable—stability. *Stratification* may be defined as the distribution of resources within a subsystem, including such elements as the number of members, their relative power, alliance ties between members, and an aggregate judgment as to the number of power centers on military-strategic questions. Analytical criteria determine the nature of the entities, so some variables will record the intuitive ratings upon which the study is based. Data on alliances have been provided by Singer and Small, and judgmental assessments by Rosecrance will be used in order to discover with which phenomena they are most closely associated. *Instability* of subsystems is empirically defined as the number of wars of various types. The total number of variables is 68, so to achieve an overview of the data multivariate procedures will be applied. First of all, the independent and dependent variables will be separately factor analyzed to ascertain the empirical structure for each set of measures; through this means we will know whether there are distinct types of stratification and instability, thus broadening the scope of the original bivariate question of a relationship between stratification and instability into a task of determining which kinds of stratification are related to what kinds of instability. Secondly, the dependent and independent variables will be analyzed together in order to discover whether the power structure of an international subsystem in fact is a concomitant of one or another type of instability in the behavior of members of the subsystem. The cross-running of dependent and independent variables will be handled by factor analysis, smallest space analysis, and cluster analysis. All three of these techniques are taxonomic in character, inasmuch as they describe clusterings of data; none actually "test" hypotheses in the classic manner prescribed by textbooks on inferential statistics, but they have the virtue of identifying close associations between variables which remain when all other variables are being held constant and, therefore, are among the most powerful tools of descriptive statistics.

## II. IDENTIFYING INTERNATIONAL SUBSYSTEMS

An *international system* may be defined as an aggregation of all politically autonomous and semiautonomous societal systems; any subset of such entities, thus, constitutes an *international subsystem*.[10] As James Rosenau has suggested, however, there can be as many systems as "issue areas."[11] In testing the stability-polarity hypothesis, hence, we are not interested in just any system; our attention is directed toward systems containing *military-strategic* confrontations between poles of actors. To regard Ecuador-Ethiopia as a relevant subsystem would be as much a potpourri choice as to select all national actors for the year 453, whether they knew of each other's existence or not; we need precise criteria for determining membership in an international subsystem which are phrased in terms of how member states relate to one another militarily.

Harold Lasswell's notion of *arena*, accordingly, will be utilized in order to locate groups of states that interact with each other on strategic and political matters.[12] When there was little interaction of any kind between states in China, Europe, India, and among tribal groups in Africa and the Americas, international arenas relevant to military-strategic issues were isolated from each other and more easily identifiable.[13] Today penetration by thermonuclear giants into the affairs of miniscule states seems to be taken for granted. Nevertheless, it is axiomatic that Africa has had little impact upon international politics among European states; moreover, only a few European countries ventured into Asia to establish a military presence, and such countries as France and the United States were able to entertain similar policies toward the Hawaiian Islands while in sharp disagreement on relations with Mexico in the 1860's. Such considerations lead to the conclusion that actors within military-strategic arenas in Europe, Asia, Africa and other regions of the globe are by no means identical. Within international regions, power distributions and configurations may differ both over time and from one part of the globe to another. The network of all military-strategic interactions among militarily autonomous units, hence, would have a number of subclusters at any one time point, and the denser parts of the network would constitute *international military-strategic subsystems*. But we need guidelines for determining which subclusters may qualify as international military-strategic subsystems.

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[10] A common semantic trap is to say that a "system" exists only when it reaches some sort of threshold level in frequency of social interactions. Certainly it is more consistent with general systems analysis to define a *system* as any set of parts that are related in some way; and a classification of all types of relations would clearly be incomplete without the category "interacting," for which one could assemble data to differentiate systems interacting at high and low levels. Entities, in short, are to be described with reference to conceptual variables, instead of being defined mystically in terms of thresholds. Such an approach is consistent with the "constructivist" systems approach advocated by David Easton, *A Framework for Political Analysis* (Englewood Cliffs: Prentice-Hall, 1965), pp. 30–34.

[11] Rosenau, *op. cit.*

[12] Harold D. Lasswell and Abraham Kaplan,

Following a set of criteria developed by Michael Brecher for locating boundaries between regional clusters of states,[14] we may identify international subsystems in terms of three elements:

I. Scope is delimited, with primary stress on a geographic region.
II. There are at least two actors.
III. There is a relatively self-contained network of political interactions between the members, involving such activities as goal attainment, adaptation, pattern maintenance, and integration, and dealing with power relations and military interactions.

These criteria are to be applied in section III of this paper to the extraction of 21 subsystems from Europe, Asia, and Hawaii, although many more subsystems are possible. The best approach would be to analyze communication networks relevant to conflict. Unfortunately relevant data for mapping military-strategic interactions are not available for long sweeps of time. A second-best approach for determining boundaries between international military-strategic subsystems is to devise specific criteria for sorting members from nonmembers of subsystems, once the subsystems have already been identified in an intuitive manner.

In delineating *members* on the basis of their participation in arenas of military conflict it is important to avoid the legalistic trap of including only so-called sovereign states, especially in an age when contests involving such entities as the Vietcong guerrillas could ultimately threaten the very survival of the human race. Adapting Singer's and Small's suggestions for selecting members,[15] the following criteria for determining membership in an international political-strategic subsystem would seem provisionally useful:

I. A member must interact more or less continuously in a manner relevant to political-military goal attainment, adaptation, pattern maintenance, and integration.
   (a) The existence of diplomatic interaction neither confers nor denies membership per se, but when an actor displays considerable diplomatic interaction with another actor, the two are members of the same subsystem.
   (b) An entity entering into an alliance gains membership in the same subsystem as the other alliance partners.

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*Power and Society* (New Haven: Yale University Press, 1950), pp. 78–80, 252–54.

[13] William McNeill, *A World History* (New York: Oxford University Press, 1967).

[14] Michael Brecher, "International Relations and Asian Studies: The Subordinate State System of Southern Asia," *World Politics,* XV (January, 1963), 221–35. For a more complete justification for the criteria, which are three fewer than Brecher's original formulation, see Michael Haas, *International Conflict* (Indianapolis: Bobbs-Merrill, forthcoming), Ch. IX. The concept of *subordinate* system, which seems to have meaning only when applied to a set of actors that are satellites of some superordinate state, thus is not relevant here.

[15] Singer and Small, *op. cit.,* use two criteria—diplomatic recognition by France or England, and a population of 500,000 or more. These criteria would of course exclude the Vietcong and other actors which, though subnational, are clearly militarily independent and internationally active due to their confrontation with countries outside of the state of which they are supposedly a part. See their "The Composition and Status Ordering of the International System," *World Politics,* XVIII (January, 1966), 236-82; Bruce M. Russett, Singer, Small, "National Political Units in the 20th Century: A Standardized List," this REVIEW, LXII (September, 1968), 932–51.

  - (c) Members must have a minimal level of internal political organization. (A rebel political faction may qualify, but a social group or ethnic community is excluded.)
- II. A member must possess military viability.
  - (a) All units with a population exceeding 500,000 are members, though smaller units may qualify.
  - (b) All sovereign states engaging in war in the relevant region are members.
  - (c) Membership ceases when an actor is absorbed or conquered and occupied.
  - (d) A separatist faction is a member (1) if it is a participant in an initially internal war in which one or more major and middle powers have intervened, or (2) if it eventually succeeds in achieving political autonomy as a separate state.

*Either* characteristic, if present, would suffice for an entity to qualify as a member of an international military-strategic subsystem. Such criteria would encompass states that have been militarily neutral for centuries, though diplomatically in a particular arena, such as Sweden in Europe; piratical bands would be excluded; and a country like the United States would date its entry into North American subsystems as soon as rebel armies acted independently of Great Britain. International subsystems, thus, consist of sets of entities meeting criterion I or II and interacting with each other in the same military-strategic arena.

*Time boundaries* for historical eras containing distinct parties and issues in conflict remain to be determined. Power stratification and alliance confrontations hardly remain constant over time, and the impact of certain wars between major powers has often been to transform the stakes of conflict dramatically among countries.[16] Specifically, we may use Rosecrance's notion of *polarity* and Kaplan's concept of the *tightness* or *looseness* of a system[17] to identify a subsystem transformation when *either* of the following two conditions occurs:

- I. A new system begins if there is a change in the number of poles, such as the following:
  - (a) a major power is demoted through military defeat,
  - (b) one or more middle powers rise to major power status,
  - (c) a major power enters the system anew or exits from the region,
  - (d) new military alliances involving major powers arise,
  - (e) existing military alliances involving major powers collapse or membership reshuffles.
- II. A new system begins when a dramatic event precipitates a trend toward tightening or loosening in power distribution, such as the following:
  - (a) a previously nonaligned power becomes an exclusive ally of another major power,
  - (b) an alliance among major powers develops a schism,
  - (c) many nonmajor powers become attached to, or break off from, bloc arrangements,
  - (d) pluralistic security communities wax or wane.

The demotion of France from the rank of major power in 1871, and Germany's dénouement in 1918, thence enable us to fix definite endpoints for old systems and starting points for new subsystem eras, just as the disruption of unity among the major powers of Europe and Hawaii were system-transforming events in 1822 and 1759, respectively.

For the reader who would prefer a quantitative approach to the identification of subsystems, cluster analysis, smallest space analysis, or factor analysis could be performed from a matrix of military-strategic interaction targets for a large number of years. Frank Denton, in undertaking a P-factor analysis of wars since the second decade of the nineteenth century, has, for example, supported Rosecrance's dating of historical subsystems.[18] Had his study failed to

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[16] The "stakes of conflict" criterion is proposed by Stanley Hoffmann, "International Systems and International Law," *World Politics*, XIV (October, 1961), 205–37; cf. Robert L. Rothstein, "Power, Security and the International System," paper presented to the American Political Science Association Annual Convention, Chicago, September, 1967; Hoffmann, *The State of War* (New York: Praeger, 1965).

[17] Kaplan, *op. cit.* For a clarificatory essay, see Wolfram F. Hanrieder, "The International System: Bipolar or Multibloc?" *Journal of Conflict Resolution*, IX (September, 1965), 299–308.

[18] Frank Denton, "Some Regularities in International Conflict, 1820–1949," *Background*, IX (February, 1966), 283–96. Denton analyzes data on wars collected by Lewis F. Richardson, *Statistics of Deadly Quarrels* (Chicago: Quadrangle, 1960). The most exact validation procedure for subsystems defined in terms of poles and looseness would consist of an R-analysis of the number of poles recorded for each year in the years to be studied and a measure of the extent of looseness (or failure to pull all members of a system into one or more of the contending poles). Ordinal data to

TABLE 1. INTERNATIONAL SUBSYSTEMS

| Region | Dates | Poles | Tightness | Availability of Resources | Members | Major Powers | Satellites | Regulator Strength | Level of Disturbance |
|---|---|---|---|---|---|---|---|---|---|
| I. Western Europe | 1649–1713 | 2 | 0 | 1 | 26 | 4 | 1 | 1 | 2 |
| II. Europe | 1714–1789 | 7 | 0 | 1 | 29 | 7 | 1 | 0 | 1 |
| III. Europe | 1790–1814 | 2 | 1 | 0 | 28 | 5 | 16 | 0 | 3 |
| IV. Europe | 1815–1822 | 6 | 1 | 1 | 32 | 6 | 2 | 2 | 1 |
| V. Europe | 1823–1847 | 2 | 0 | 0 | 39 | 6 | 2 | 1 | 2 |
| VI. Europe | 1848–1871 | 5 | 0 | 0 | 38 | 5 | 3 | 0 | 3 |
| VII. Europe | 1872–1890 | 1 | 0 | 1 | 19 | 6 | 0 | 2 | 2 |
| VIII. Europe | 1891–1918 | 2 | 1 | 0 | 33 | 7 | 1 | 0 | 3 |
| IX. Europe | 1919–1945 | 2 | 0 | 0 | 45 | 6 | 13 | 0 | 3 |
| X. North Atlantic | 1946–1963 | 2 | 1 | 1 | 32 | 3 | 10 | 2 | 2 |
| XI. East Asia | 1689–1842 | 1 | 0 | 1 | 7 | 1 | 1 | 0 | 1 |
| XII. East Asia | 1843–1895 | 2 | 1 | 1 | 15 | 4 | 1 | 1 | 2 |
| XIII. East Asia | 1896–1913 | 2 | 0 | 0 | 13 | 3 | 1 | 1 | 3 |
| XIV. East Asia | 1914–1945 | 3 | 1 | 1 | 20 | 1 | 2 | 0 | 3 |
| XV. East Asia | 1946–1954 | 2 | 1 | 0 | 18 | 2 | 3 | 2 | 3 |
| XVI. Asia | 1955–1963 | 3 | 1 | 1 | 32 | 2 | 4 | 2 | 2 |
| XVII. Hawaii | 1738–1758 | 3 | 0 | 0 | 5 | 3 | 0 | 1 | 1 |
| XVIII. Hawaii | 1759–1782 | 3 | 1 | 0 | 9 | 3 | 1 | 0 | 3 |
| XIX. Hawaii | 1783–1795 | 2 | 1 | 0 | 8 | 2 | 1 | 0 | 3 |
| XX. Hawaii | 1796–1818 | 1 | 1 | 1 | 5 | 1 | 1 | 1 | 1 |
| XXI. Hawaii | 1819–1898 | 1 | 0 | 1 | 4 | 1 | 1 | 0 | 1 |

support the intuition of a highly knowledgeable scholar, however, the criteria would not have been changed drastically, for some sort of analytical cuts are always prior to the selection of data for analysis. It is important to justify these cuts theoretically, as we have done, and to obtain precise estimates of the extent to which one's analysis is procrustean, a task to be discussed below in section IV.

## III. EUROPEAN, ASIAN AND HAWAIIAN SUBSYSTEMS

Specifying territorial and time boundaries for international subsystems in practice involves a great deal of trial and error experimentation. Whether to decide first upon members or upon years presents a chicken and egg dilemma. The most painless procedure has been to posit "core members" of subsystems, and then to fix an arbitrary starting point in time.

For Europe the most obvious core members are France, England, Spain, Prussia (Germany), and the former Austrian Hapsburg Empire. The date of 1649 could be selected as a starting point because in that year the Peace of Westphalia had concluded the Thirty Years War by redrawing European boundaries. But in order to transcend the parochialism of a completely European sample, additional subsystems are to be carved out. China and Japan are central members of any East Asian subsystem. We may begin our analysis in 1689, when China signed its first modern treaty (with Imperial Russia), the effect of which was to slow down Russian expansion in Asia. Until the unification of the Hawaiian Islands by King Kamehameha I in 1810, the various rulers of the islands Hawaii, Oahu, and Maui were at the helm of the most significant members of Hawaiian subsystems. Foreign penetration, which did not occur until the late eighteenth century, was undertaken by France, Great Britain, Russia, and the United States. A lack of earlier records dictates as an initial year the choice of 1738, when a conference between the kings of Oahu, Maui and Hawaii resulted in a ceasefire of a war that might otherwise have meant the unification of the islands under the joint rule of Alapainui of Hawaii and his brother, Kamehamehanui of Maui.

Many other subsystems could have been chosen—African, American, Middle Eastern, and South Asian, for example. In this preliminary analysis a small sample, 21 subsystems in all, is preferred so that familiarity with each case would be maximized while at the same time there would be enough cases for multivariate manipulation of the data. The subsystems to be analyzed are presented in Table 1, along with a summary of all codings judgmentally assigned in accordance with our criteria for detecting poles and members of subsystems. Figures for number of major powers, members, and satellite states

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construct such indicators is not easily at hand, and since we have already assigned 0 and 1 codings implicitly in the discussion to follow, an R-analysis of nominal scale codings would doubtless be a trivial exercise.

are based on the Singer-Small lists, as modified by membership criteria presented in section II of this paper. Poles, availability of resources, regulator strength, and level of disturbance codings are taken from Rosecrance for subsystems II–X and supplemented in a manner that seems consistent for the remaining eras.[19] Although detailed descriptions of the eras appear elsewhere,[20] a brief summary is presented here to justify the judgmentally assigned codings and to substantiate the analytical cuts (in space and time) for each of our 21 subsystems.

Rosecrance's European subsystems may be capsulized as follows: (I) *Western Europe 1649–1713,* Louis XIV opposed by coalitions on three occasions; (II) *Europe 1714–1789,* evanescent alliances among jockeying major powers; (III) *Europe 1790–1814,* revolutionary France establishes a system of satellite states until finally checked by the Quadruple Alliance; (IV) *Europe 1815–1822,* five major powers maintain a tight concert of Europe reign; (V) *Europe 1823–1847,* France-England and Prussia-Russia-Austria maintain separate spheres of influence; (VI) *Europe 1848–1871,* major powers disband the concert and their spheres of influence; (VII) *Europe 1872–1890,* alliance network with nexus at Berlin, France isolated; (VIII) *Europe 1891–1918,* antagonism between Triple Alliance and Triple Entente; (IX) *Europe 1919–1945,* France sets up an alliance framework isolating Germany until Hitler establishes the Axis pole; (X) *North Atlantic 1946–1963,* Soviet and Western blocs engage in a cold war struggle.

Since the Asian and Hawaiian subsystems are not discussed in any previous source, our description is more detailed:

(XI) *East Asia, 1689–1842.* After the abortive Russian Amur Expedition of 1689, the East Asian international subsystem was reaffirmed to be unipolar in character, though in the later years of China's long hegemony in the Far East her supremacy was assumed loosely to exist without a concerted attempt to test her strength. China was the only major power, with Japan and Russia as abdicating middle powers.

(XII) *East Asia, 1843–1895.* The easy victory of England in the Opium War changed the East Asian power situation dramatically. Although capitalist states were interested primarily in trading privileges, decentralization of power throughout China encouraged Western powers to cooperate in a tight bipolar struggle to enlarge "spheres of interest" in China. The Opium War brought France, Great Britain and Russia to major power status. The Netherlands, newly colonizing in the East Indies, and Spain, with control in the Philippines, constituted new middle powers linked to the bipolar struggle of East versus West. And the Western powers began to assume an implicit regulatory role in East Asia.

(XIII) *East Asia, 1896–1913.* When Japan unexpectedly proved victorious in its war with China in 1894–95, France, Germany, and Russia, exercising an explicit regulatory role, compelled Japan to revise the terms of the Treaty of Shimonoseki, thereby creating an independent Korea and reducing the likelihood that Japan might use some clauses of the treaty as a shoehorn to continental domination. Bipolarity resulted. The absorption of Great Britain, Germany and the United States with matters outside East Asia meant a loose, rather than a tight stratification of power. Russia served as the protector of China, and France's European-oriented alliance with Russia constrained any aggressive Asian ambitions the latter might have contemplated. Insofar as Russia's cooperation with China appeared to tip the scales on the side of a future Russian hegemony in Asia, the British became disturbed and formed the Anglo-Japanese Alliance in 1902, which initially encouraged Japan to tackle Russia in 1904, but the Japanese were content to accept American mediation in 1905 for a settlement restoring a loose bipolarity in East Asia. China and Russia were demoted to middle power status due to their wartime defeats in 1895 and 1905, respectively, and the United States entered the system as a middle power upon unseating Spain from the Philippines in 1898.

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[19] I wish to express my gratitude to Professors Charles Hunter, Werner Levi, and Norman Meller for offering assistance in my analysis of the Asian and Hawaiian materials. The scarcity of scholars able to assess my extraction of subsystems in Asia and Hawaii precluded a Delphi procedure such as the one apparently undertaken by Rosecrance. See Rosecrance, *Action and Reaction in World Politics, op. cit.*, p. x; Olaf Helmer, *Convergence of Expert Consensus Through Feedback* (Santa Monica: Rand, 1964). I have altered the dates of some of Rosecrance's subsystems so as to eliminate overlapping years, thereby enabling a smoother data collection procedure.

[20] M. Haas, *loc. cit.* The European eras are most succinctly described in Mowat, *op. cit.;* David Jayne Hill, *A History of Diplomacy in the International Development of Europe* (New York: Longmans, Green, 1914); and Rosecrance, *Action and Reaction in World Politics, op. cit.* For Asia, see Harold M. Vinacke, *A History of the Far East in Modern Times* (6th ed.; New York: Appleton-Century-Crofts, 1959); Harley F. MacNair and Donald F. Lach, *Modern Far Eastern International Relations* (2nd ed.; New York: Van Nostrand, 1955). The major sources on Hawaii are Abraham Fornander, *An Account of the Polynesian Race* (vol. II; London: Trubner, 1880); Ralph S. Kuykendall and A. Grove Day, *Hawaii* (rev. ed.; Englewood Cliffs: Prentice-Hall, 1961); Kuykendall, *The Hawaiian Kingdom, 1854–1874* (Honolulu: University of Hawaii Press, 1953); Sylvester K. Stevens, *American Expansion in Hawaii, 1842–1898* (Harrisburg: Achives Publishing, 1945). A more monographic presentation would be preferable, so the reader should consult sources cited in these works for more complete accounts.

(XIV) *East Asia, 1914–1945.* The outbreak of World War I in Europe provided an unparalleled opportunity for Japan to increase its power in the East Asian subsystem without regulatory countermoves. As of 1917 both Russia and China ceased to be unified entities within the system, and the Western powers were consumed first with the European world war, and with internal economic reconstruction afterwards. Japan agreed at the Washington Conference of 1921 to a distribution of power in which it would be "balanced," but the 5:5:3 ratio in naval strength left Japan as the number one power in East Asia. The Manchurian invasion of 1931 and the subsequent establishment of the puppet state Manchukuo were contested only verbally. Japanese forces moved southward to conquer China in 1936. Aid for the Chinese was provided by Moscow, reenforcing a tripolar constellation composed of Japan, Russia and China, and as yet uncommitted Britain, France, and the United States. After the Pearl Harbor bombing in late 1941, Japan was opposed by nearly every other power in Asia, though there was no joint allied command.

(XV) *East Asia, 1946–1954.* The immediate postwar period consisted of a bipolar contest. The United States established a military presence in Japan and Okinawa, while the increased power of the USSR in Asia became a new factor. Victory by the Chinese Communists over the Kuomintang was finally achieved by 1949, and the system tightened in 1950 with the outbreak of the Korean War, the American and UN regulatory response to which underscored a policy of containment of Communism.

(XVI) *Asia, 1955–1963.* Seeds of a three-cornered alignment were sown in 1951, when United Nations' forces crossed the 38th parallel into North Korea over the objections of India. Delhi, a middle power in South Asia, withdrew her support from the United Nations' action and began to proselytize on behalf of a policy of nonalignment. The independence of several states in Southeast Asia, and French withdrawal from Indochina and India by 1954, played into India's hands as the leader of a new grouping of states. Following Indonesia's acceptance of a neutralist foreign policy and the Bandung Conference of 1955, tripolarity was established on firmer ground. With the Indochinese buffer gone, the East Asian subsystem merged with the formerly separate South Asian international subsystem. A period of peace was guaranteed by the cohesiveness of neutralists, activity of the UN, and parity between the power blocs of East and West. Escalating participation of the United States in Vietnam, possession of thermonuclear weapons by China, the Sino-Indian Border War, Pakistan's estrangement from the American-sponsored SEATO pact, and the rift between Malaysia and Indonesia brought an end by 1963 to the tripolar house of cards constructed by the neutralist bloc.

(XVII) *Hawaii, 1738–1758.* The forces of Alapainui, ruler of the island of Hawaii, nearly succeeded in unifying all of the major Hawaiian Islands in the year 1738. Alongside his brother, who was ruler of Maui, Alapainui's combined military force subdued Molokai and sailed for Oahu, where victory was almost certain but would be contested bitterly by Peleioholani, ruler of Oahu. Rather than pursuing what might be a pyrrhic victory, the three rulers agreed to a cessation of hostilities with Oahu and concluded the Treaty of Naonealaa, establishing an informal "concert," whereby any new war would be halted at the insistence of the third. A relatively peaceful era, with a loose trifurcation of power, followed.

(XVIII) *Hawaii, 1759–1782.* The Kapalipilo Battle between Maui and Hawaii ushered in an era of bellicose leaders, each militantly seeking to unify all of the islands. A Hawaii military expedition invaded Maui, successfully annexing the eastern section of the island. Maui, led by Kahekili, turned its sights on both Oahu and Hawaii, the former for conquest and the latter for revenge. A tight tripolar struggle ensued until 1782, when Maui conquered Oahu. Hawaii, meanwhile, was convulsed with internal discord. The heir to Kalaniopuu threatened to redistribute land in a manner particularly unattractive to the chiefs of the Kona district, who preferred Kamehameha, and a civil war followed for a ten year period.

(XIX) *Hawaii, 1783–1795.* After the victory of Maui over Oahu, Kahekili took up residence on Oahu, creating a power vacuum in the middle of the island chain. Kamehameha secured control of Maui, Molokai, and Lanai in 1790, even before his authority had been solidified on his home island, and the Kahekili-Kamehameha fight was continued up to 1795, when in the Battle of Nuuanu the army of Kamehameha pushed its opponent's forces up to the Pali precipice; those who escaped plunging to their death did not contest the new hegemony of Kamehameha. All of the islands except Kauai and Niihau fell under the rule of Kamehameha I.

(XX) *Hawaii, 1796–1818.* Though Kamehameha's unipolar preeminence clearly dates from 1796, the existence of a last independent ruler spurred Kamehameha to sail to Kauai. After heavy storms forced his fleet of warships to turn back on two occasions Kamehameha softpedelled the urgency of a victory that seemed inevitable. His patience was rewarded when Kaumaulii, the ruler of Kauai, declared a voluntary surrender in 1810. When a group of Russians, who had been encouraged by Kaumaulii to establish a colony on Kauai in 1796, departed in 1818, Kamehameha I assumed complete dominance.

(XXI) *Hawaii, 1819–1898.* The position of Kamehameha and his successors loosened considerably during the nineteenth century, though unipolar control was maintained. At various times such middle powers as France, Great Britain, and the United States contemplated forcing Hawaii into an imperialistic cocked hat. France moved first in 1819, but soon realized that conquest would not be so easy. In 1843 an entente was reached between France and Great Britain, in which it was agreed that Hawaii should remain independent. The American president John Tyler was reluctant to go along with the 1843 agreement for fear that the specter of secret treaties, power politics, American imperialism and foreign "entanglement" would be vilified by the opposition party. In the last half of the century American and Oriental settlers arrived in great numbers, gradually taking over the commercial and economic control of the islands. In 1887 the United States gained a naval leasehold over Pearl Harbor, thus elevating Washington to the status of a major power, and two years later American marines were landed at the request of Kamehameha V in order to insure that the so-called Wilcox Revolt would fizzle. The monarchy fell in 1893 primarily at the instigation of the American settlers; a republic was declared in 1894; and the islands were annexed by joint resolution of Congress in 1898, terminating the autonomy of Hawaiian subsystems.

### IV. DATA ON STRATIFICATION AND STABILITY

Our main aim of determining whether types of power distributions are related to the incidence of international conflict, requires that we assemble objective, quantitative data measuring the extent of power stratification as well as the extent of war participation within each of the analytically derived subsystems. We now turn to data on alliances and wars.

Data on international stratification, our independent variable, are available to us in the form of membership and polarity codings and alliance formation statistics. The alliance data, which cover only the period 1815–1945, have been collected principally by Singer and Small.[21] Table 2 lists 33 variables indexing the power structure of each of our 21 subsystems. Variables 1–5 deal with the number of poles; variable 1 is recoded as a series of dichotomies in variables 2–5. Rosecrance's codings comprise variables 6–8. Membership assignments are presented in variables 9–15. Variables 16–19 represent gross counts of the number of alliances, their size, and the extent to which members of the subsystem are tied together by alliances. Variables 20–23 trace the extensiveness of alliance links between major and minor powers, thus yielding a measure of hierarchy in alliance formation, and variables 24–27 measure the extensiveness of the practice of major power alliances that are made exclusively with each other. Variables 28–33 represent measures of *procrusteanness.* It would be ideal in terms of our criteria to find that alliances are all terminated at the conclusion of a subsystem and new ones are begun as a subsystem starts. But this is not the case. Variables 28–30 measure the degree to which our dating of subsystems fails to coincide with the natural lives of alliances among members of each system. Geography, similarly, may be a source for procrustean slicing of subsystems, so variables 31–33 index the extent to which countries within a particular subsystem enter into alliances beyond the spatial confines of the main continent in which the subsystem is centered.

The variables might appear to be independent observations on the extent of stratification within an international arena. An analysis with 33 independent variables, however, would hardly be parsimonious. If subsets of the variables empirically collapse into highly intercorrelated clusters, our analysis would be simplified. Accordingly, a *factor analysis* of the alliance data is reported in Table 2.[22] Unfortunately the data

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[21] Singer and Small, "Alliance Aggregation and the Onset of War, 1815-1945," *loc. cit.* I have supplemented these data with a compilation of alliances between members of the five Hawaiian subsystems based on Fornander, *loc cit.* Definitions of each variable appear in the Appendix to this paper. Data are available upon request from the author.

[22] Since none of the variables depart significantly from normal curve distributions, though they tend to be rectangular in view of the small sample size, product-moment correlations were computed be-

TABLE 2. POLARITY AND ALLIANCE DATA

| | Source[a] | Cases | $h^2$ | Unrotated Factors | | | | | | | | | Quartimin Factors[b] | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | I | II | III | IV | V | VI | VII | VIII | IX | I | II | III | IV | V | VI | VII | VIII | IX |
| 1. Number of poles | R/H | 21 | .99 | 44 | −41 | −21 | (−51) | 36 | −18 | −22 | 27 | −13 | 04 | 42 | 05 | −26 | 29 | (96) | 24 | 37 | 24 |
| 2. Unipolarity | R/H | 21 | .88 | −38 | −16 | −35 | 24 | −(59) | 09 | 30 | 02 | −29 | −07 | −40 | −03 | −38 | 08 | −30 | −49 | (61) | (59) |
| 3. Bipolarity | R/H | 21 | .94 | 45 | (51) | 26 | 45 | 07 | 31 | −05 | −03 | 29 | 41 | 23 | −03 | (91) | −04 | (−50) | 24 | −31 | (−55) |
| 4. Tripolarity | R/H | 21 | .79 | −40 | −13 | 35 | −41 | 39 | −08 | 08 | −32 | −24 | −36 | 01 | 05 | (−61) | 02 | −03 | 23 | (−53) | 33 |
| 5. Multipolarity | R/H | 21 | 1.01 | 45 | −42 | −43 | −40 | 11 | −21 | −24 | 40 | −04 | 09 | 26 | 03 | −15 | 24 | (101) | 02 | (63) | 12 |
| 6. Availability of resources | R/H | 21 | .86 | −27 | −44 | −32 | 40 | 03 | −49 | 15 | 01 | −25 | −16 | 29 | −08 | (−73) | −39 | 01 | 01 | 37 | (51) |
| 7. Tightness of poles | R/H | 21 | .98 | −19 | 08 | −15 | 29 | (85) | 22 | 25 | −01 | −07 | −15 | 29 | (−62) | −21 | 07 | −36 | (80) | −05 | 08 |
| 8. Strength of regulators | R/H | 21 | 1.00 | 33 | (−51) | 07 | 36 | 05 | 12 | (68) | −02 | 16 | 20 | 18 | 39 | −37 | 25 | (−52) | 45 | −03 | −32 |
| 9. Number of members | H | 21 | .92 | (80) | 28 | −28 | −12 | 11 | −26 | 07 | −16 | −00 | (85) | 35 | −10 | −00 | 03 | 29 | −04 | −34 | −04 |
| 10. Number of major powers | H | 21 | 1.03 | (88) | 02 | −16 | −19 | −09 | 16 | −10 | −14 | −33 | (68) | (61) | 11 | 13 | (50) | 33 | −23 | −00 | (54) |
| 11. Number of major & middle powers | H | 21 | .94 | (87) | −04 | −10 | 16 | 09 | −28 | −16 | −15 | −14 | (66) | (88) | 13 | 07 | −04 | 28 | −03 | −21 | 23 |
| 12. Number of satellites | H | 21 | .96 | 34 | (74) | 06 | −05 | 13 | −37 | 37 | 11 | −02 | (80) | −38 | −16 | 11 | −30 | 04 | 43 | 27 | 07 |
| 13. Fully sovereign members/members | H | 21 | .96 | 01 | −48 | (−65) | −09 | −38 | 17 | −20 | −26 | 10 | −17 | 18 | −10 | −27 | 32 | 13 | (−91) | (−51) | −22 |
| 14. Members/middle & major powers | H | 21 | .77 | 13 | (53) | (−51) | −22 | 06 | −09 | 30 | 07 | 24 | (51) | (−60) | (−50) | −01 | 01 | 12 | −02 | −04 | −42 |
| 15. Major powers/major & middle powers | H | 21 | .94 | 05 | 10 | −08 | −49 | −07 | (73) | 13 | 15 | −30 | 00 | −36 | −12 | 13 | (88) | 09 | −01 | (55) | (53) |
| 16. Number of alliances | SS/F | 15 | 1.01 | (55) | (74) | 04 | −08 | −21 | −24 | 11 | −03 | −18 | (91) | −15 | −03 | 31 | −15 | 11 | −06 | 18 | 35 |
| 17. Aligned countries/members | SS/F | 15 | .99 | 41 | −12 | (77) | −09 | −34 | −28 | −04 | −01 | 06 | 22 | 12 | (91) | 22 | −19 | 07 | 04 | −10 | −12 |
| 18. Alliance partners/alliances | SS/F | 15 | .87 | (61) | (−53) | 02 | −06 | −02 | −37 | 14 | 01 | 23 | 30 | 39 | (54) | −30 | −01 | 31 | 09 | −23 | −43 |
| 19. Alliance partners/alliances/members | SS/F | 15 | .91 | −23 | −42 | (73) | −22 | −20 | −17 | 09 | 05 | 15 | −41 | −26 | (80) | −17 | −13 | −02 | 15 | −08 | −27 |
| 20. Number of alliances between major and minor powers | SS/F | 15 | .94 | (58) | (70) | −03 | −12 | −23 | −13 | 13 | 04 | −09 | (89) | −24 | −04 | 38 | −03 | 14 | −07 | 22 | 18 |
| 21. Minor powers allied to major powers/minor powers | SS/F | 15 | .98 | (87) | 29 | −24 | −09 | −18 | 01 | 03 | 07 | 19 | (81) | 10 | 05 | 44 | 21 | 27 | −15 | −02 | −33 |
| 22. Partners in alliances including minor & major powers/all such alliances | SS/F | 15 | .99 | (91) | −13 | −26 | 09 | 18 | 06 | 10 | 07 | 11 | (62) | (58) | 05 | 15 | 32 | 21 | 22 | −00 | −22 |
| 23. Partners in alliances including both minor & major powers/all such alliances/minor powers | SS/F | 15 | .86 | (57) | −17 | 09 | (52) | 18 | 32 | −12 | 23 | 15 | 13 | (68) | 12 | (62) | 20 | −15 | 40 | 26 | −25 |
| 24. Number of alliances exclusively between major powers | SS/F | 15 | .92 | (69) | (50) | 09 | 21 | −21 | 14 | −17 | −15 | −15 | (70) | 41 | 03 | (65) | 08 | −12 | −21 | −09 | 25 |
| 25. Alliances exclusively between major powers/alliances including major powers | SS/F | 15 | .97 | (65) | −33 | (53) | 06 | −17 | 28 | 12 | −10 | −17 | 28 | (54) | (72) | 16 | 43 | −20 | 14 | −03 | 25 |
| 26. Partners in exclusive major power alliances/all such alliances | SS/F | 15 | 1.00 | (86) | −34 | 13 | −15 | 04 | −02 | 25 | 15 | −09 | (52) | 42 | (52) | −09 | 41 | 31 | 35 | 32 | 15 |
| 27. Partners in exclusive major power alliances/all such alliances/major powers | SS/F | 15 | .99 | (67) | −40 | 48 | −16 | −03 | 05 | 16 | 27 | −09 | 26 | 30 | (75) | 08 | 36 | 27 | 46 | (52) | 17 |
| 28. Alliances starting in earlier era or ending in later era/alliances | SS/F | 15 | .93 | 49 | (−53) | −27 | 42 | −23 | −12 | −01 | −30 | −01 | 24 | (79) | 26 | −25 | 02 | −14 | −40 | −44 | −02 |
| 29. *Alliances including both major & minor* powers starting in earlier era or ending in later era/alliances including major and minor powers | SS/F | 15 | .92 | (67) | −45 | −19 | 35 | 19 | 13 | 13 | −15 | −13 | 33 | (92) | 11 | −18 | 33 | −12 | 16 | −14 | 19 |
| 30. Alliances exclusively between major powers starting in earlier era or ending in later era/alliances exclusively between major powers | SS/F | 15 | .69 | (54) | −08 | −02 | 27 | −13 | 14 | −47 | 17 | 15 | 13 | (58) | 15 | (72) | 06 | 21 | −16 | 12 | −23 |
| 31. Extraregion alliances/all alliances | SS/F | 15 | .95 | (−61) | 07 | −03 | (56) | −04 | −42 | −09 | 20 | −14 | −34 | −08 | −21 | −09 | (−72) | −14 | 08 | (64) | 38 |
| 32. Extraregion alliances including major powers/all alliances including major powers | SS/F | 15 | .83 | −41 | 06 | −18 | 38 | −40 | 11 | 18 | (51) | 11 | −18 | (−57) | −11 | 17 | −09 | −14 | 02 | (130) | 36 |
| 33. Extraregion alliances including minor powers/all alliances including minor powers | SS/F | 15 | .99 | 28 | 24 | 39 | (64) | 30 | −08 | −31 | 10 | −31 | 15 | (82) | −01 | (54) | −34 | −16 | 49 | 41 | (60) |
| Eigenvalues | | | | 10.3 | 5.1 | 3.6 | 3.3 | 2.4 | 2.1 | 1.7 | 1.2 | 1.0 | | | | | | | | | |
| Per cent total variance | | | | 31.2 | 15.4 | 10.9 | 10.0 | 7.3 | 6.4 | 5.2 | 3.6 | 3.0 | | | | | | | | | |
| Per cent common variance | | | | 33.6 | 16.6 | 11.7 | 10.7 | 7.8 | 6.8 | 5.5 | 3.9 | 3.3 | | | | | | | | | |

[a] Key: R/H=Rosecrance, Haas; H=Haas; SS/F=Singer and Small, Fornander.

[b] P matrix.

do not condense themselves as much as we could wish, for a total of nine factors emerge, which is quite unusual for so few variables. The first two factors account for about half of the total variance of all of the variables, but the remaining factors taper off so gradually that we must include them in any complete description of the elements of stratification within international subsystems.

The most important factor[23] is clearly a *Size* factor, inasmuch as variables counting the total number of alliances, members, and of major-minor alliances consistently receive high loadings along with numbers of members, major powers, and most other variables given in gross magnitude form. The most significant element of an international subsystem emerging from 33 measures of stratification, then, is whether it has many or few members and alliances, in keeping with the primacy attributed to the variable of size by Deutsch and Singer. European subsystems are large, Asian systems medium, and Hawaiian systems small, and this basic dimension is unrelated to the eight other facets of stratification. The second factor is *Procrusteanness.* Such a large representation of variables 28–33 in this factor indicates that subsystems differ greatly in the extent to which they contain alliances that are a legacy of the past or that will endure despite power reshufflings, as opposed to subsystems with alliances made only specific to the power confrontation of the period, on the one hand, or cosmopolitan members who contract alliances with members in other subsystems versus those subsystems that are insular in character, on the other hand. The high loadings for number of major and middle powers indicates that activity outside the system is more likely when there are many states potentially competing for topdog status. Some of our cuts made in time and space, in short, were much better than others, and since procrusteanness is an important dimension of the data, we are fortunate in being able to assess the exact extent to which it is related to aspects of foreign conflict delineated in the next section of this paper. Factor III is an *Alliance Saturation* dimension, since it groups together the per cent of countries aligned and the multilaterality of alliances. This dimension can be used in a retest of the hypothesis that alliances sometimes deter wars, and at other times serve as a prelude to wars. The fourth factor refers to *Blocs,* containing as it does high loadings for bipolarity, unavailability of resources, major-minor alliances, as well as a tendency for alliance patterns among the major powers to remain solid from one subsystem era to the next. Waltz' adulation for the success of the contemporary cold war in preventing various disturbances from disrupting the stability of international relations can be examined more carefully with this dimension of stratification, which corresponds closely with Rosecrance's picture of a bipolar world. *Differentiation* emerges as a fifth factor. Subsystems evidently differ significantly in the presence of a layer of middle powers intervening between major and minor powers, and a low number of alliance ties on the part of intrasystem members with countries in other subsystems. A situation in which there is a wide gulf between powerful and weak members is thus related to the geographic isolation of a subsystem; and, conversely, subsystems that are penetrated by outside powers tend to contain almost as many middle and major powers. The fact that *Multipolarity* appears as a sixth factor, however, demonstrates that interaction opportunities are unrelated to the tendency for systems to contain many or few poles. The fact that alliance membership, size, and multipolarity are unrelated undercuts one of the assumptions in the Deutsch-Singer formulation. The per cent of all actors that are satellite states or militarily active subnational groups attracting the attention of outside powers was the basis for calculating a percentage of nonsovereign members, which loads high and negatively on the seventh factor, along with the tightness of a subsystem, and it seems intuitively reasonable to expect fewer amorphous entities in a loosely structured system. Accordingly, we shall refer to this factor as *Tightness,* even though the nonsovereign variable has a somewhat higher loading.

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tween the 33 variables. A principle axis factor analysis, with 1.0, was run, and there was a negative eigenvalue on the 14th cycle. An orthogonal (varimax) rotation, after determining the number of factors from the eigenvalue 1.0 criterion, approximated simple structure, so a quartimin rotation was performed, using an oblimin program developed by John Carroll; gamma was set at 0.0, and the solution ran 30 cycles, 13,500 iterations. A biquartimin rotation was tested but, in conformity with the observation that varimax was very close to simple structure, factor loadings were so greatly inflated in value as to be meaningless.

[23] Factor number assignments coming from the unrotated solution often do not correspond with oblique solutions; the unrotated factors, which are brought into a clearer form through an orthogonal rotation, denote a succession of factors starting with the most important (in terms of variance explained) and decreasing to the least important. Here we follow the numbering of the quartimin factors, for the sake of clarity in reading the results.

The interfactor correlations among the first seven factors are all below ±.50, so they are not presented in a table here, but some are above ±.40: (1) procrustean systems are seldom bloc systems, though they are tight; (2) blocs are generally tight. Factors VIII and IX, however, collapse almost completely, for they correlate with each other at the −.85 level, and they are correlated with factors II, IV and V at the .70 level or higher. The much lower percentages of variance explained by the two final factors indicates that they may be tapping some random error in the data.[24]

Turning to the dependent variable, international stability, it is possible to utilize a number of lists of wars, their duration, casualties, and countries participating in each instance of international violence. Lewis F. Richardson's *Statistics of Deadly Quarrels* is the most detailed in coverage, for it includes many subnational entities and internal wars as well as the more traditional case of hostilities between separate, legitimate sovereign states, though there are no data before 1820, after 1950, or concerning the Hawaiian islands. It will be necessary to supplement the list, therefore.[25] A second source is Quincy Wright's *A Study of War*, which counts the number of wars with "formal declarations" for a much longer time period, but his compilation tends to ignore subnational participants.[26] Because findings could be highly dependent upon the way in which data are collected in each study, one way of proceeding is to conduct two separate analyses, one for each source. Table 3 contains variables collected from Richardson, and Table 4 lists similar variables from Wright. Independent analysis with the two sets of data will permit replication and thus strengthen the overall conclusions from this study.

In Table 3 the indicators fall into several categories. Variables 34–39 sum war data across all countries; the same types of indicators are next counted for major powers only in variables 40–44. The effect of wars is dealt with in variables 45–47, with *destabilization* defined as victory in a war on the part of the aggressor, which can be determined from the description of each quarrel in Richardson's compilation. Extrasystem warfare is indexed by variables 48–51, and Rosecrance's "level of disturbance" coding is included as variable 52 in order to ascertain what sorts of quantitative data are most applicable. A factor analysis of the data, also reported in Table 3,[27] is much less complex in structure than the alliance data. Only 6 factors emerge from the 19 variables, the first two factors accounting for about 60 per cent of the total variance, and the high communality ($h^2$) estimates reveal that there is little unexplained variance among the variables. The factors are themselves easier to name.

Factor I is a *War Incidence* dimension, with high loadings for many wars and many new wars for both major and all other powers. International subsystems, hence, differ greatly in the amount of violence present, some being much more peaceful than others, but the gross extent of violence is independent of five other dimensions of war. Factor II is a *Multilateral* factor, with high codings for war deaths, Rosecrance's coding for disturbance, and with the tendency for wars to have many rather than few participants. The strength of a regulator, thus, is most taxed in world wars and in major confrontations among several states but is unrelated to the

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[24] Factor VIII seems to indicate Imperial Expansion; IX, Imperial Decline.

[25] Richardson, *op. cit.*, pp. 33–111. The supplementary sources are Fornander, *loc. cit.*, and data supplied by Mr. Richard Cady, Bendix Systems Division, Ann Arbor, from a study of low-intensity warfare sponsored by the Office of Naval Research, Contract NC0014–66–CO262.

[26] Quincy Wright, *A Study of War* (2nd ed.; Chicago: University of Chicago Press, 1965), pp. 644–49. In view of his more formal criteria it would not appear consistent to add Hawaii data to his list; for the same reason I have deducted subnational members from the total number of members in norming variable 54, as well as 17 and 19. Two other sources, Singer and Small, and Sorokin, employ restrictive criteria that made their lists less useful for the particular purposes of this study: Singer and Small do not include interventions, and their data pertain only to 1815–1945; Sorokin assembles data only for about 10 countries, though over a very long time period. Singer and Small, "Alliance Aggregation and the Onset of War, 1815–1945," *loc. cit.*; Pitirim A. Sorokin, *Fluctuations of Social Relationships, War and Revolution* (New York: American Book Company, 1937), pp. 547–77.

[27] A principal axis factor analysis was performed on the 19 × 19 correlation matrix with unities in the diagonal. Six factors emerged with eigenvalues over 1.0, and negative eigenvalues were encountered on the ninth cycle. An orthogonal (varimax) rotation was next run, and the subsequent biquartimin solution (30 cycles, 9000 iterations) was virtually identical in higher loadings with the orthogonal solution though much cleaner with regard to intermediate loadings. Accordingly, only the biquartimin P matrix is presented in Table 3. The only high interfactor correlation is —.60, between factors I and V.

TABLE 3. RICHARDSON DATA

| Variable | Source[a] | Cases | $h^2$ | Unrotated Factors |  |  |  |  |  | Biquartimin Factors[b] |  |  |  |  |  |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
|  |  |  |  | I | II | III | IV | V | VI | I | II | III | IV | V | VI |
| 34. Number of wars | R/F/B | 16 | .94 | (81) | −35 | −15 | 02 | 37 | 09 | (89) | 15 | 09 | 16 | 10 | −06 |
| 35. Members at war/members | R/F/B | 16 | .95 | (76) | 06 | −20 | 34 | −19 | −42 | 14 | 45 | 10 | (52) | (−56) | −13 |
| 36. Participants in wars/members at war | R/F/B | 16 | .82 | (87) | −22 | −04 | 07 | −07 | −09 | 46 | 23 | 01 | 48 | −15 | 10 |
| 37. Participants in wars/wars | R/F/B | 17 | .91 | (76) | 27 | 26 | 39 | −11 | 16 | 12 | (76) | 14 | 09 | 03 | 36 |
| 38. Durations of wars/wars | R/F/B | 16 | .95 | (70) | −04 | 30 | −46 | −35 | −21 | −02 | −12 | 44 | (74) | 03 | 20 |
| 39. Years with new wars/years in era | R/F/B | 18 | .96 | (76) | 17 | −30 | −47 | 23 | 08 | (73) | −14 | (56) | −02 | −01 | 06 |
| 40. Number of wars with major powers | R/F/B | 16 | .95 | (71) | −40 | −36 | 05 | 35 | 17 | (97) | 05 | −21 | 02 | −02 | −02 |
| 41. Wars with major powers/major powers | R/F/B | 17 | .86 | (68) | −12 | −31 | 17 | −25 | 45 | (53) | 19 | −17 | −09 | −22 | (61) |
| 42. Participants in wars with major powers/all such wars | R/F/B | 17 | .88 | (66) | 28 | 33 | 38 | 24 | −24 | −03 | (72) | 10 | 08 | 06 | (50) |
| 43. Durations of wars with major powers/all such wars | R/F/B | 16 | .93 | (63) | 10 | 31 | −36 | −47 | 27 | −02 | −02 | 35 | 33 | 14 | (68) |
| 44. Years with new wars with major powers/years in era | R/F/B | 18 | .98 | (76) | −01 | −46 | −37 | 21 | 13 | (86) | −19 | 34 | −04 | −14 | 08 |
| 45. Wars resulting in destabilization/wars | R/F/B | 16 | .95 | (69) | −47 | 31 | −08 | 07 | −38 | 28 | 08 | −04 | (85) | 13 | −25 |
| 46. Wars with major powers resulting in destabilization/wars with major powers | R/F/B | 17 | .98 | 38 | (−78) | 32 | 02 | −31 | −17 | 03 | −10 | −49 | (89) | 08 | 09 |
| 47. War deaths/wars | R/F/B | 11 | 1.17 | 24 | 48 | 25 | (87) | 22 | −10 | −15 | (109) | −01 | −20 | −02 | −15 |
| 48. Wars starting in earlier era or ending in later era/wars in era | R/F/B | 18 | .95 | 45 | (72) | 06 | −44 | 02 | −17 | −01 | 07 | (96) | 00 | −03 | 00 |
| 49. Wars with major powers starting in earlier era or ending in later era/wars with major powers in era | R/F/B | 17 | .92 | 40 | (77) | 09 | −38 | 10 | −11 | −00 | 14 | (95) | −12 | 04 | −01 |
| 50. Extraregion wars/all wars | R/F/B | 14 | .99 | −10 | −15 | (84) | −14 | 22 | 42 | −08 | 10 | −09 | −06 | (98) | 26 |
| 51. Extraregion wars with major powers/all wars with major powers | R/F/B | 16 | .91 | −01 | −16 | (74) | −29 | (50) | −06 | 05 | 00 | 14 | 22 | (85) | −31 |
| 52. Level of disturbance | R/H | 21 | .93 | (79) | 15 | 17 | 37 | 32 | −07 | 42 | (72) | 15 | 12 | 10 | −11 |
|  |  |  | Eigenvalues | 7.8 | 3.4 | 2.8 | 2.6 | 1.6 | 1.2 |  |  |  |  |  |  |
|  |  |  | Per cent total variance | 41.1 | 17.9 | 14.7 | 13.7 | 8.4 | 6.3 |  |  |  |  |  |  |
|  |  |  | Per cent common variance | 40.2 | 17.5 | 14.4 | 13.4 | 8.2 | 6.2 |  |  |  |  |  |  |

[a] R/F/B = Richardson, Fornander, Bendix.
R/H = Rosecrance, Haas.

[b] P Matrix.

TABLE 4. WRIGHT DATA

| Variable | Source[a] | Cases | $h^2$ | Unrotated Factors | | | | | Biquartimin Factors[b] | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | I | II | III | IV | V | I | II | III | IV | V |
| 52. Level of disturbance | R/H | 21 | .62 | 37 | (52) | 45 | 03 | −09 | (78) | −17 | 10 | −03 | −06 |
| 53. Number of wars | W | 16 | .93 | (80) | −21 | −38 | −19 | 24 | 02 | (93) | 10 | 18 | 07 |
| 54. Members at war/members | W | 16 | .79 | (68) | 17 | 47 | −13 | −24 | (75) | 11 | −17 | 06 | −31 |
| 55. Participants in wars/members at war | W | 16 | .93 | (79) | −38 | −20 | −07 | 33 | 04 | (85) | −08 | 37 | 15 |
| 56. Participants in wars/wars | W | 16 | .91 | (66) | 43 | (50) | −16 | 10 | (93) | 18 | −05 | −03 | 09 |
| 57. Durations of wars/wars | W | 16 | .93 | (62) | −44 | 10 | (58) | 08 | 07 | 15 | −03 | (90) | −12 |
| 58. Years with new wars/years in era | W | 16 | .77 | 25 | (53) | (−62) | 06 | −18 | −03 | 24 | (79) | −14 | −18 |
| 59. Number of wars with major powers | W | 16 | .95 | (78) | −33 | −31 | −28 | 24 | 00 | (96) | −07 | 14 | 07 |
| 60. Wars with major powers/major powers | W | 16 | .83 | (68) | (−54) | 12 | 07 | −25 | 10 | 33 | −31 | 48 | −45 |
| 61. Participants in wars with major powers/all such wars | W | 16 | .94 | (53) | (58) | (54) | −14 | 08 | (97) | 03 | 01 | −12 | 11 |
| 62. Durations of wars with major powers/all such wars | W | 16 | .91 | (66) | −44 | 04 | (59) | 06 | 02 | 16 | 01 | (90) | −14 |
| 63. Years with new wars with major powers/years in era | W | 16 | .79 | (59) | 12 | −43 | −49 | −06 | 09 | (79) | 19 | −32 | −12 |
| 64. War deaths/wars | SS/B | 12 | .92 | (68) | 43 | 44 | 22 | 16 | (86) | 05 | 16 | 31 | 12 |
| 65. Wars starting in earlier era or ending in later era/wars | W | 16 | .88 | 12 | (68) | (−52) | 36 | −04 | 04 | −03 | (94) | 04 | −00 |
| 66. Wars with major powers starting in earlier era or ending in later era/wars with major powers | W | 16 | .87 | 01 | (70) | −45 | 39 | 14 | 06 | −07 | (90) | 05 | 20 |
| 67. Extraregion wars/all wars | W | 21 | .90 | (−79) | −28 | 40 | 03 | 18 | −25 | (−56) | (−51) | −07 | 29 |
| 68. Extraregion wars with major powers/all wars with major powers | W | 21 | .91 | −44 | 24 | 09 | −06 | (81) | 01 | 01 | −01 | −20 | (94) |
| Eigenvalues | | | | 6.1 | 3.4 | 2.7 | 1.5 | 1.2 | | | | | |
| Per cent total variance | | | | 35.9 | 20.0 | 15.9 | 8.8 | 7.1 | | | | | |
| Per cent common variance | | | | 40.9 | 22.8 | 18.1 | 10.1 | 8.0 | | | | | |

[a] Key: R/H = Rosecrance, Haas.
W = Wright.
SS/B = Singer and Small, Bendix.

[b] P Matrix

number of total wars. Theorists who are willing to abide many wars but cannot tolerate large losses of life will be more interested in factor II than factor I in the subsequent analysis. Factor III consists of two main *Extra-Era* variables, and factor V is an *Extra-Region* dimension. Unlike the pileup of procrusteanness indicators in the case of alliances, the time and space variables appear here on different factors. *Destabilization* appears as factor IV. Evidently long wars are largely capable of achieving the aspirations of aggressive states. The close association between destabilizing wars and subsystems with many war participants evokes an image in which countries gang up on powers that are on the decline, such as the Ottoman Empire in nineteenth century Europe. But the −.60 correlation between factors I and IV suggests that wars in eras with destabilization are likely to be infrequent. The final factor contains a cluster of *Major Power Wars*. International subsystems containing many major powers at war, to summarize at this point, are equally likely to have many or few total wars, extrasystem wars, war casualties, stabilization or destabilization as a consequence.

Our next question is whether a similar clustering can be found in Quincy Wright's data, which is exhibited in Table 4. Nearly all of the indicators are identical in form with those collected from Richardson, to facilitate such a replication, but Wright provides no destabilization clues and no figures for war deaths. As a partial substitute, casualty estimates collected by Singer and Small, which correlate +.79 with those of Richardson but are based on criteria more similar to Wright than to Richardson, are inserted. Evidently because it was impossible to use supplementary data for destabilization, only five factors are extracted from the 17 Wright variables. The first two factors again account for about 60 per cent of the total variance. Several variables, however, fail to enter into the results, especially Rosecrance's disturbance level.[28]

*Multilaterality* is the first factor, with variables pertaining to the number of participants per war receiving higher loadings than war casualties, unlike the corresponding factor for Richardson's data. The fact that "participants in wars/wars" and "participants in wars/members at war" end up on different factors may seem paradoxical, but evidently belligerents in particular wars are sometimes recidivists and at other times newly play the role of war participant. One cannot predict from the presence of mainly dyadic or multilateral wars whether the subsystem has had many or few total wars. *War Incidence* is factor II for Wright; unlike the same dimension in Richardson's data, there is a stronger linkage with the number of different countries entering war. Some subsystems, in other words, have few wars and few countries at war, while other subsystems are arenas in which nearly everly member fights on the battlefield and a new war breaks out in many years of the total life of the era. Few members enter wars outside such turbulent subsystems, perhaps due to the excessive preoccupation with events in the wartorn region. *Extrasystem Wars* are found together on factor III. Although factor V does contain a high loading for *Extraregional Major Power Belligerency*, this final factor is a unique one, is the least important in terms of variance explained; it does not collapse into factor II, with which it is correlated −.70. Factor IV is a *Duration* dimension. Richardson's data contained a factor in which variables relating to length of wars also clustered, but we labelled that factor "destabilization" because the two variables loading most significantly were measures of the tendency for aggressors to upset the status quo. Lacking such indicators for the Wright data, we should not be surprised to find this result.

One factor missing from the factor analysis of Wright's war data is the sixth factor from Richardson, namely, Major Power Wars. Wright's focus on wars that have formal declarations and sovereign participants does of course weight the sample in the direction of major power wars, in contrast with Richardson's enumeration of wars including such entities as the Portuguese Miguelites and Vietnamese insurgent groups before the independence of the Vietnam republics.

To sum up this section of the discussion, we have collected 68 indicators of international stratification and instability, and factor analyzed the independent and dependent variables separately. Stratification variables are grouped together in seven major clusters: Size, Procrusteanness, Alliance Saturation, Blocs, Differentiation, Multipolarity, and Tightness. (Imperial Expansion and Decline emerge as the final two factors.) To determine relationships between international power stratification and stability these seven to nine factors, hence, must be juxtaposed with Richardson's and Wright's data on wars. For Richardson the respective dimensions of international stability are War Incidence, Multilaterality, Extra-Era, Destabilization, Extraregionality, and Major Power Wars. Wright's data contain all but the last of these factors, though the clusters are somewhat differently formed and ordered (Mulilteratality, War Incidence, Extrasystem Wars, Duration, Extraregional Major Power Belligerency).

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[28] The Pearsonian $17 \times 17$ matrix input to a principal axis factor analysis with 1.0 in the diagonal. Five factors emerged with eigenvalues over 1.0, and a negative eigenvalue terminated the run on the eleventh cycle. An orthogonal (varimax) rotation was performed and a subsequent biquartimin rotation took 30 cycles, 16469 iterations. The only high interfactor correlation is −.70, between factors III and V.

### V. STABILITY AS A FUNCTION OF POWER STRATIFICATION

With seven to nine dimensions of the independent variable and five to six dimensions of the dependent variable, an inventory of the total findings of this study might fill 50 or so cells of an interfactor correlation matrix, which is hardly a parsimonious prospect, though of course slightly more so than a 68 × 68 matrix of intervariable correlations. We have elected to pool the variables together, therefore, in order to determine whether some of the original power stratification factors collapse into the major dimensions of international stability.

A factor analysis of the dependent and independent variables together might seem to be the most obvious course available. There are some pitfalls in using factor analysis in hypothesis testing, however. The end product of factor analysis can only be a set of vectors, each of which contains variables whose high intercorrelations remain even when all other variables are held constant; thus, factor analysis is ideal for determining when the null (no-relation) hypothesis is correct, since each factor will be noncorrelated with every other. But even when looking at simple correlations, a high correlation beckons us to be suspicious that the relationship is between two variables that are fundamentally the same anyway, and thus is a trivial one. For the same reason the results of a factor analysis are likely to ferret out only the most obvious sorts of likenesses between dimensions. But, if we seek to test a hypothesis involving variables that are from substantially different theoretical realms, we will want a technique that is intermediate in power between the simple correlation coefficient and the extreme rigor of factor analysis; a technique that scales or sorts out close from weak relations might be better suited for our purposes. Accordingly, smallest space analysis and cluster analysis are to be employed below.[29]

Richardson's data are examined along with the stratification variables, using both smallest space analysis and factor analysis. Patterns in the latter, as expected, are often difficult to discern, and thus are not presented here.[30] Few of the nine dimensions of the independent variable either remain intact or collapse along with one of the six dimensions in Richardson's war data. The first factor, however, does juxtapose Multipolarity with the War Incidence factor. Multipolar systems, thus, have many wars involving major as well as minor powers; there are many new wars per year in a multipolar system, and many different countries enter these wars.

Our attention is directed instead at the results from *smallest space analysis* (Table 5 and Figure 1).[31] In contrast with the factor analysis, the picture in Figure 1 portrays considerable proximity between variables. Looking more closely at the main independent variables, one sees unipolarity to the far left, tripolarity on the far right, multipolarity at the bottom, and bipolarity embedded in the north-central portion of the configuration. We may infer that each type of power distribution is uniquely associated with

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[29] Louis Guttman, "A General Nonmetric Technique for finding the Smallest Coordinate Space for a Configuration of Points," *Psychometrika,* XXXIII (December, 1968), 469–506; Joe H. Ward, Jr., "Hierarchical Grouping to Optimize on Objective Function," *American Statistical Association Journal,* LVIII (March 1963), 236–44. The specific code names are SSA-I for the former, and Cluster IV for the latter.

[30] The 52 × 52 correlation matrix in the diagonal. Twelve factors were extracted with eigenvalues greater than 1.0, and negative eigenvalues appeared on the fourteenth cycle. An orthogonal (varimax) rotation was performed but so many communalities exceeded 1.0 (due to missing data) that it was impossible to obtain a meaningful oblique solution.

[31] The storage limitation for smallest space analysis on the IBM 7040 at the University of Hawaii Computing Center was 50 variables, so the stratification data (33 variables) were reduced by two variables in the analysis with Richardson's data (19 variables). The excluded variables were number 1 (number of poles), which has emerged on the same factor with multipolarity in every run attempted with factor analysis, and variable 27, which has similarly been so related with another variable (number 26) as to be superfluous in a larger analysis. Isobars in the SSA-I figures are drawn around variables on the basis of loadings ±.50 in the factor analyses reported in Tables 2–4. Lower case Roman numerals refer to the stratification factors; upper case, to Richardson's dimensions in Figure 1, and to Wright's dimension in Figure 2. Labels are not attached to each variable number, since the names given in Tables 2–3 are so lengthy that they would clutter the plot unesthetically.

TABLE 5. TWO-DIMENSIONAL SMALLEST SPACE ANALYSES

| Variable Number | Phi Coefficients |  | Variable Number | Phi Coefficients |  |
|---|---|---|---|---|---|
|  | Run 1 | Run 2 |  | Run 1 | Run 2 |
| 1 |  | .03 | 35 | .02 |  |
| 2 | .03 | .02 | 36 | .01 |  |
| 3 | .02 | .02 | 37 | .01 |  |
| 4 | .07 | .05 | 38 | .01 |  |
| 5 | .02 | .03 | 39 | .01 |  |
| 6 | .03 | .03 | 40 | .01 |  |
| 7 | .03 | .04 | 41 | .03 |  |
| 8 | .03 | .02 | 42 | .01 |  |
| 9 | .01 | .01 | 43 | .01 |  |
| 10 | .01 | .01 | 44 | .01 |  |
| 11 | .01 | .01 | 45 | .01 |  |
| 12 | .02 | .01 | 46 | .02 |  |
| 13 | .04 | .04 | 47 | .04 |  |
| 14 | .03 | .03 | 48 | .02 |  |
| 15 | .05 | .03 | 49 | .01 |  |
| 16 | .02 | .01 | 50 | .01 |  |
| 17 | .03 | .04 | 51 | .01 |  |
| 18 | .01 | .01 | 52 | .01 | .02 |
| 19 | .05 | .04 | 53 |  | .02 |
| 20 | .02 | .01 | 54 |  | .02 |
| 21 | .02 | .01 | 55 |  | .03 |
| 22 | .01 | .01 | 56 |  | .01 |
| 23 | .01 | .01 | 57 |  | .01 |
| 24 | .02 | .02 | 58 |  | .02 |
| 25 | .01 | .01 | 59 |  | .02 |
| 26 | .01 | .01 | 60 |  | .04 |
| 27 |  | .01 | 61 |  | .02 |
| 28 | .01 | .01 | 62 |  | .02 |
| 29 | .01 | .01 | 63 |  | .02 |
| 30 | .02 | .02 | 64 |  | .01 |
| 31 | .03 | .02 | 65 |  | .02 |
| 32 | .02 | .02 | 66 |  | .03 |
| 33 | .02 | .01 | 67 |  | .02 |
| 34 | .01 |  | 68 |  | .01 |
|  |  |  | Iterations | 37 | 25 |
|  |  |  | Normalized Phi | .05 | .04 |
|  |  |  | Coef. Alienation | .30 | .28 |

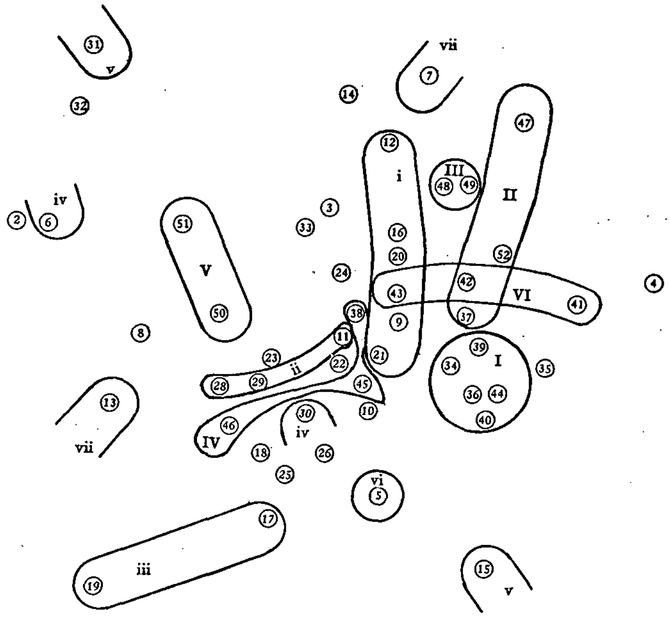
some variables, but much less so for other variables; following Rosecrance, we may read the figure in terms of the respective costs for each.

*Unipolarity* is surrounded mainly by stratification variables, such as resource availability and extraregionality of alliances, which is reminiscent of the Imperial Decline and Expansion factors in the preliminary analysis of the 33 independent variables. If we search for a cost of unipolarity in terms of warfare, the only relevant variables are extraregional wars. *Bipolarity,* though apparently related to a larger number of variables, in view of its centrality in the plot, in actuality is located amid stratification variables. A bipolar system will evidently encourage minor powers to make alliances in other subsystems; there will be many alliances of all types; many satellites and middle and minor powers will be present. The main systemic cost is the tendency for longer wars, which often spill over into adjacent eras, with major powers frequently drawn into military confrontations. *Tripolarity* proves to be very explosive and is associated much more with war than with stratification variables. Tripolar systems will have more countries simultaneously fighting on the battlefield; there will be many ongoing wars and many new wars each year. Clearly the tripolar systems, which in our sample appear in modern Asia and pre-unification Hawaii, are much more eventful and crisis-ridden than bipolar systems. *Multipolarity* is associated most closely with such remaining stratification variables as exclusive major power alliances and indicators of Alliance Saturation. Destabilization and War Incidence are the main costs found in the widest arc around the multipolarity variable that does not intrude completely into the domains of bipolarity and tripolarity.

The dichotomization of types of power distribution into four variables according to number of poles evidently produces a configuration in which each of the four main types of polarities are at opposite ends from one another. With the exception of bipolarity, so few war variables are close to these extremes that alliance variables seem more predictive of international stability. As isobars in Figure 1 reveal, variables loading together at high levels on the respective factor analyses are found in adjacent locations in the smallest space plot. Among the alliance factors there is a tendency for the less important (low eigenvalue) dimensions to be situated on the left of the configuration and to be unrelated to war variables; the 19 Richardson variables are more widely scattered, though mostly on the right hand side. (Partial bounding by an isobar denotes that the other member of the relevant factor loads negatively and therefore is located at the opposite end of the plot.)

Variables indexing system size are located from north to side in the middle of the figure, virtually bisecting factors derived from Richardson's data. On the basis of the closest factors one may infer that a tight subsystem with many alliances, members, and minor powers allied to major powers is very likely to have many powers actively engaged in wars; the wars will be long, often spilling over into adjacent

FIGURE 1. Smallest Space Analysis of Richardson and Stratification Data[a]



[a] For key to variable numbers, see Tables 1 and 2. Upper case Roman numbers denote oblique factors from Table 3; lower case Roman numbers, oblique factors from Table 2.

eras, with many lives lost. In larger systems, there will be many outbreaks of warfare, especially among major powers, and the wars are likely to result in destabilization, that is, victory for powers trying to upset the status quo. The Deutsch-Singer interaction opportunity hypothesis, in short, is completely inconsistent with the findings of the smallest space analysis: we should look for smaller systems, with less so-called interaction potential, to bring about a more peaceful state of affairs.

Turning to the independent variables, which are bunched together at the south-central portion of the plot, we find that when members of a subsystem contract many alliances, both at home and in other regions, there is a tendency for these systems to have rival blocs. Such arrangements deserve the label *superblocs*, for the alliance tentacles extend to many arenas. The consequence of superblocs is for upsets of the status quo in both home and foreign regions, while War Incidence, Duration and Number of Countries involved in each war are much less distinctly related to the presence of superblocs. Most of the remaining stratification dimensions are more isolated from war variables. Alliance Saturation in a system breeds only Destabilization; Imperial Expansion and Decline are related to Extraregional Wars; a Differentiated system, as defined by a parity in major and middle powers and extraregionality in alliance partnerships, is unrelated to war variables. These latter two sets of variables merge together into a single Procrustean cluster. Because the characteristics used to define our 21 subsystems in space and time were stratification variables, it is possible to infer that the main patterns discovered in Richardson's war data are unrelated to the procrusteanness of our original analytical cuts on alliance formation patterns. Instead, wars did not fit our temporal and spatial boundaries very well, contrary to Denton's analysis.

Although the dimensions of Richardson's data resemble those of Wright's, the two sources fail to overlap insofar as Richardson's data, even when expanded by the Bendix and Fornander sources, do not pertain to subsystems I-V or XI; Wright's coverage is complete for all but Hawaiian subsystems. A smallest space analysis of the stratification and Wright variables, some 50 in all, promises to unravel any artifacts in the results due to sampling problems. The most prominent feature of the second smallest space analysis[32] is a wider amount of scatter among the points (Table 5 and Figure 2), but the overall goodness of fit of the two-dimensional solution is almost identical with the Richardson data. The four main war dimensions are found at the extreme right, whereas stratification clusters once again enjoy a much wider ambit. Some stratification variables, hence, are more related to war than others.

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[32] A factors analysis of the 33 stratification and 17 Wright variables once again produced results that were unsatisfactory in technical respects—inflated communalities, failure of an oblique solution to improve simple structure from an othogonal solution. A principal axis factor analysis was first

The costs and benefits of various types of polarities resemble those found with the Richardson data. *Unipolarity* is considerably isolated from the independent variables in the northwest portion of the figure; the only war variables nearby are extraregional war indices. Members of unipolar systems, in other words, tend to fight outside the region rather than inside, as had been discovered in Figure 1. *Bipolarity*, although once again planted squarely in the middle of the configuration, occupies a point surrounded largely by independent variables, and it is least distant from variables indexing wars with many belligerents. *Tripolarity* is the same distance from the War Multilaterality factor as bipolarity but closer to variables measuring the length and numbers of wars. *Multipolarity*, finally, is adjacent to the Extra-Era wars and somewhat closer to the War Incidence dimension than bipolarity. Unlike Richardson's data, most of the Wright war variables are closer to the polarity codings than to the alliance data. Because of the restrictive sampling criteria employed by Wright, and the consequent overweighting of wars involving major powers, perhaps we should not be surprised to find some of these slight differences from the more inclusive Richardson data.

Some alliance and stratification factors, nevertheless, are highly related to Wright's war data. Size, Multipolarity, and Differentiation box in most of the war dimensions. Subsystem Size is fixed intimately vis-à-vis War Incidence and touches Multilaterality; Extra-Era and Extra-Region variables are near also, but multipolarity is closest to the Extra-Era cluster. Presence of middle powers and absence of neutrals, the two variables loading highest on the Differentiation factor, are most predictive of War Incidence; the two points are at opposite ends of the plot by dint of the negative loading of one of the two variables on the Differentiation dimension. Consistent with the Richardson findings, the Size dimension is unquestionably related to the War Incidence and Multilateral factors. Larger subsystems are somewhat more distant from extrasystem wars in the Wright data, undoubtedly because the latter source lists fewer colonial conflicts than Richardson. The propinquity between War Incidence and Differentiation, however, is a new finding entirely. For Wright the presence of a substantial number of middle powers and a lack of extrasystem alliances entails more wars; in Richardson's data the Differentiation variables are stationed closer to War Incidence.

Although findings in the two smallest space analyses converge in general, some incongruities do becloud generalizability. A pooling of all 68 variables seems necessary in order to determine whether the dimensions of Richardson's and Wright's data are so disparate as to warrant a more complex interpretation of results. Alternatively, we did observe considerable resemblance in the variables loading on factors extracted from the preliminary screenings of the 19 and 17 variables, so a combined analysis would seem to hold the prospect for merger of the dependent variable dimensions and hence a more simplified pattern would emerge vis-à-vis the stratification variables.

To replicate the smallest space analysis findings, a *cluster analysis* is performed.[33] The advantage of cluster analysis is that one can decompose a set of variables on a binary basis, such that the most similar pairs of variables are first grouped together and then additional clusters are added to the original atomic dyads in a tree-like fashion. Our four variables pertaining to types of power stratification were coded as 0 or 1, so we are assured that in a cluster analysis four main clusters appropriate to polarity would remain distinct until the very end; we are thus able to determine precisely which alliance and war variables have the greatest affinity for the four types of power distributions. There is another convenient feature of the cluster analysis procedure. Data from Richardson and Wright were collected so as to parallel each other: for each source, we have figures for number of wars, number of major power wars, and so on down the list. If the data are greatly different in some systematic or random manner, for example, then

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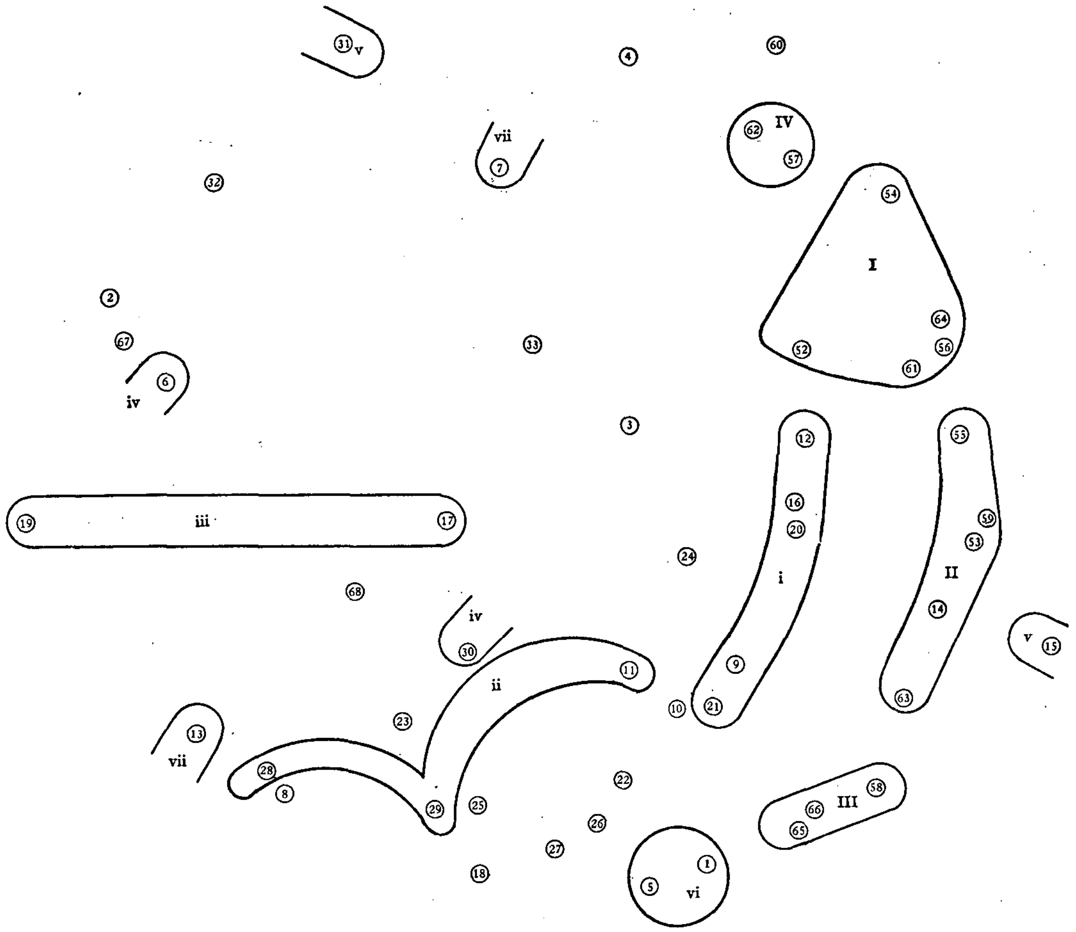
attempted with 1.0 in the diagonal, nine factors emerging with eigenvalues exceeding 1.0 and a negative root was encountered on the tenth cycle. The varimax findings, however, were somewhat sharper: (1) Multipolarity and Size collapse alone with War Incidence on the first factor; (2) Size and Multilaterality, on factor II; (3) War Incidence and Differentiation load high on factor III, but are inversely related. None of the remaining factors recombine any of the dimensions discovered in the preliminary analysis.

[33] Smallest space analysis was precluded because of the 50-variable limit. The data input to the cluster analysis was the 68 × 68 Pearsonian correlation matrix.

FIGURE 2. Smallest Space Analysis of Wright and Stratification Data[a]



[a] For key to variable numbers, see Tables 1 and 3. Upper case Roman numbers denote oblique factors from Table 4; lower case Roman numbers, oblique factors from Table 2.

we would expect that variable 34 (Richardson: number of wars) would not emerge on an early cluster with variable 53 (Wright: number of wars), and so forth for the remaining variables. Cluster analysis supplies a precise measure, the coefficient of similarity, for the extent to which any one variable resembles another, thus permitting us to assess which Richardson and Wright variables are most similar or dissimilar.

In examining the results, we find first of all that separate unipolar, bipolar, tripolar, and multipolar clusters do indeed emerge (Figures 3-6). Unipolarity eventually merges with bipolarity, and the tripolar and multipolar clusters are combined, but only after all other variables have been located on one of the four clusters.[34] Secondly, most of the variables assembled from the Richardson and Wright compilations emerge either on initial dyadic clusters or appear within second-order clusters, so the two sources are fundamentally homogeneous. And, thirdly, the initial dimensions—seven to nine for stratification variables, six and five respectively for the Richardson and Wright sources—are clearly discernible, the war dimensions collapsing into stratification dimensions as anticipated above.[35]

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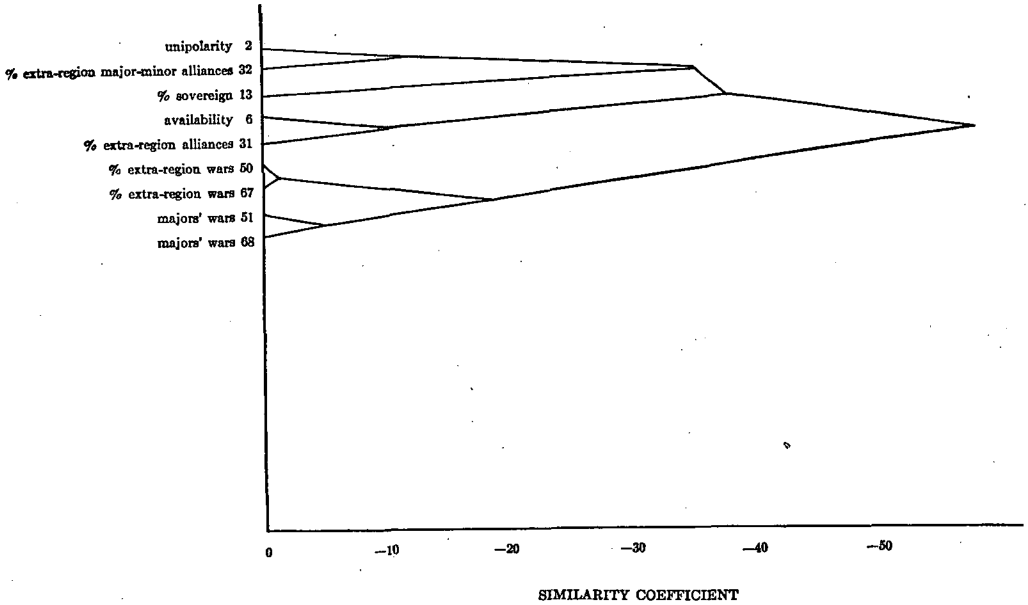
[34] The four main clusters converge as follows: the unipolar and bipolar clusters merge at —92.5356; the tripolar and multipolar clusters collapse at —102.5750; and the entire set of 68 variables come together at a similarity coefficient of —119.9040.

[35] A principal axis factor analysis on the 68 × 68 Pearsonian correlation matrix extracted seven factors, with a negative root encountered on the eighth cycle. The orthogonal (varimax) loadings

Figure 3. Unipolarity Cluster



There are two subclusters within the cluster containing *unipolarity* (Figure 3). Associated most closely with the existence of a single power center is a tendency for extraregional alliances to be contracted between major and minor powers; a large number of sovereign states enter the subcluster next, followed by an availability of resources-extraregional alliances dyadic cluster. The Imperial Decline and Expansion dimensions are thus located here. Extra-Era and Extraregional wars, factor V for both Richardson and Wright, constitute the second subcluster, and the fact that they merge with the first subcluser at such a high similarity coefficient level indicates that the two subclusters are relatively homogeneous. Unipolar international subsystems, hence, are likely to be rather dull and pacific arenas: major and even minor powers are likely to contract alliances and to make wars in other locales, though a lack of internal alliances is more intimately a facet of unipolarity than absence of wars. Results are more complex in the *bipolarity* cluster (Figure 4). Bipolar systems evidently nurture exclusive alliances among major powers, involve many minor powers in alliances with major powers, and the scope of these alliances extends beyond the analytical confines of a particular subsystem region. These were the variables loading on the Blocs dimension in the preliminary factor analysis. The next subcluster is composed of Destabilization variables: bipolar wars are very likely to be long and will result in destabilization. The larger subsystems have been bipolar, we find this time, for the third subcluster contains magnitudes in subsystem members (major, middle, minor, and satellite powers) and in alliances of all types. From the magnitude of the similarity coefficients it appears that bipolarity is more responsible for lengthy, destabilizing wars than for subsystem size. Less well connected, but more related to bipolarity than to other power distributions, is a tendency for strong regulators, multilateral alliances, multi-major alliances, and a preference on the part of major powers

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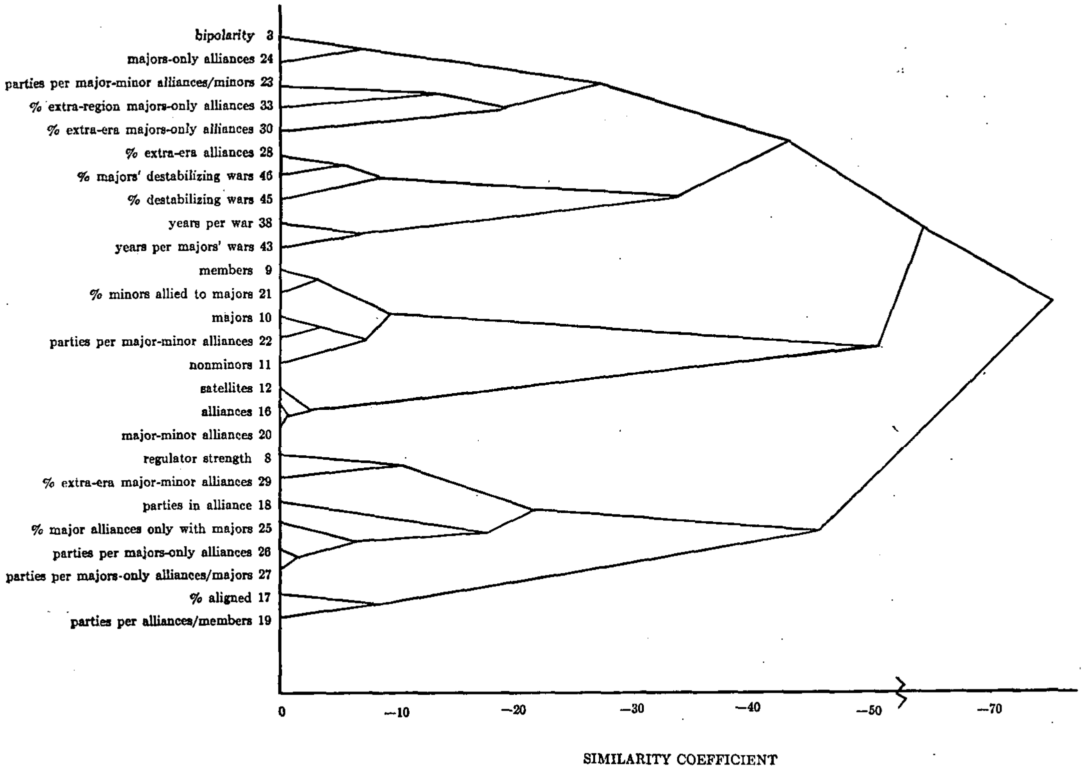
were input to a quartimin oblique rotation, which ran 30 cycles, 28500 interations, a biquartimin solution having turned up an unacceptable abundance of inflated factor loadings. The findings are similar to those derived from the cluster analysis: (1) Multipolarity and Size collapsed along with War Incidence on factor I; (2) Procrusteanness and subsystem Size are related to Extrasystem Wars; (3) Size is linked to Multilateral Wars; (4) Alliance Saturation and Extra-Era War dimensions are found together; (5) Alliance Saturation is related to Duration and Destabilization on two factors, one for Richardson and the other for Wright. The meaning of the seventh factor is not clear. These results are not presented here because they reinforce previous findings and, thus, would be redundant information; yet another reason, however, is that the polarity codings, our main interest herein, receive no significant loadings. Cluster analysis does not bury the relationships one seeks to discover and is therefore discussed in more detail.

FIGURE 4. Bipolarity Cluster



for alliances only with one another; few isolates are observed within the dense alliance network. Alliance Saturation merges with bipolarity only when the Destabilization and Size dimensions are bound up with bipolarity: Alliance Saturation, thus, is related to destabilizing and lengthy wars in larger subsystems. *Tripolar* subsystems are distinguished by their tightness (lack of neutral powers) and as well for an absence of middle powers (Figure 5). Because some of the tripolar systems are Hawaiian, and hence not reflected in Wright's data, there is some ambiguity and lack of close association between the initial subcluster and the other subclusters sketched in Figure 5. The second cluster contains both Richardson and Wright variables pertaining to the Extra-Era dimensions, but the third subcluster seems only very remotely a part of the total cluster. Since tripolarity joins multipolarity soon after the formation of the third subcluster in Figure 5, we may interpret the variables in the final tripolar subcluster, which come from War Multilaterality dimensions, as reflecting a general pattern common to any subsystem with more than two main poles: nearly every country is at war, wars tend to be general rather than localized in terms of number of participants, and many soldiers die during these eras. These findings negate the image of multipolarity as freer from dramatic crises, for the outcome of general wars is much more likely to lead to system transformations than the results of localized conflicts involving the use of force. The initial subclusters within the unipolarity, bipolarity, and tripolarity clusters are similar insofar as variables extracted from Richardson and Wright are not present; war data form distinct subclusters, which later merge with the polarity variables. Such is not the case with *multipolarity* (Figure 6), which is apparently wedded to the appearance of new wars within many years of a subsystem era. The two other subclusters, similarly, contain variables referring to a large number of total wars and warlike states, whether measured in magnitudes or normed by dividing raw figures in such a way as to render the quotients more comparable from one subsystem to the next. The War Incidence dimension, in short, is associated uniquely with multipolarity and with no other stratification variable.

## VI. DISCUSSION

Results from the above tests of the relationship between power stratification and international stability challenge much of the existing

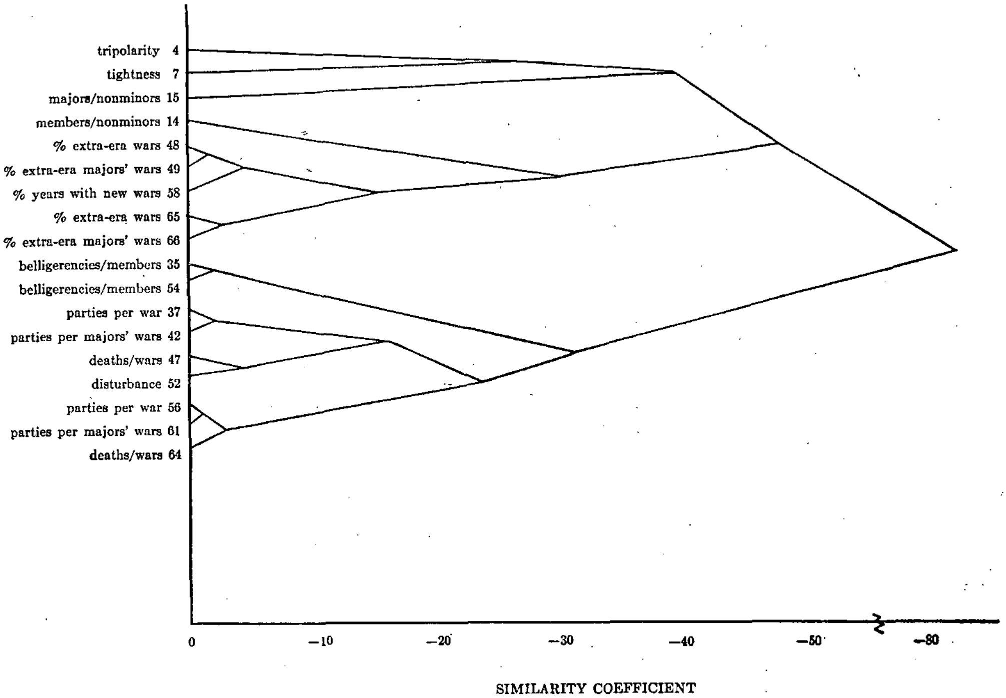


FIGURE 5. Tripolarity Cluster

folklore within current system theory in international relations. First of all, the number of poles in a system predicts somewhat less well the amount of violence than do other stratification elements, such as alliance configurations. Secondly, classical balance of power theory, with its faith in so-called inherently benign operations within multipolar systems, is undermined.

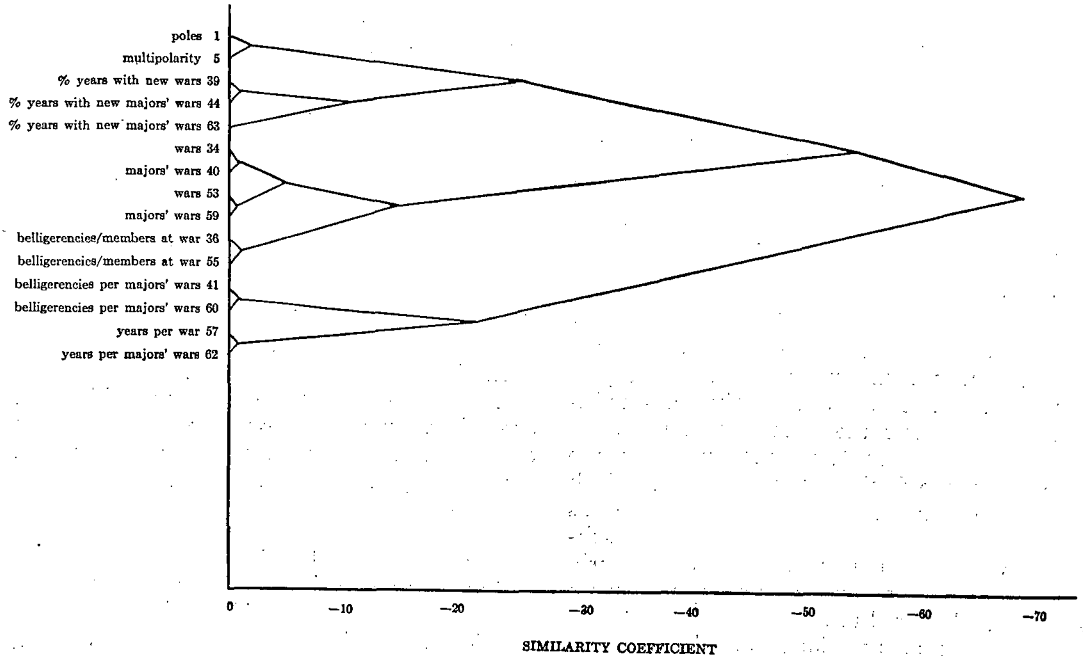


FIGURE 6. Multipolarity Cluster

Thirdly, procrustean alliance patterns are related only to extrasystem wars, so our analytical cuts do not interfere with the generalizability of our findings. Fourthly, results are substantially the same for two distinct data bases: our findings are independent of criteria for determining membership in subsystems.

*Unipolar* systems are clearly the most pacific; most members prefer to engage in war outside the system if at all. But of course a hierarchical unipolarity in which one state dominates many others is largely an extinct phenomenon historically. *Bipolarity* is associated consistently with longer wars, and countries seeking to upset an existing distribution of resources are most successful in bipolar systems. Rosecrance's hypothesis that bipolar struggles have a definite impact in terms of stabilization or destabilization is hence supported. Consistent with Waltz, major powers in bipolar systems must be content to accept infrequent, localized yet prolonged wars as concomitants of an intense rivalry that erupts in nibbling at the few ambiguous peripheries of superbloc domains. Tripolarity and multipolarity are more difficult to separate from each other in terms of systemic costs and benefits. They contain the highest rates of war, incidence of war casualties, and most countries in such systems dispatch troops to battlefields. Multilaterality is assigned most frequently to *tripolarity,* and war incidence adheres to *multipolarity.* This finding is consistent with Rosecrance's characterization of multipolarity as most likely to harbor conflict with ambiguous outcomes but negates the Deutsch-Singer formulation.

The choice between bipolar versus multipolar arrangements now seems clear. If a state or group of states is willing to accept long wars that are won by aggressor states, bipolarity provides an escape from the more war-prone, character of historical multipolar subsystems. Multipolarity entails more violence, more countries at war, and more casualties; bipolarity brings fewer but longer wars.

## VII. CONCLUSION

Several conditions bound the generalizability of the findings presented herein. Although most of the current theorizing on international system dynamics is based upon European cases, and the present study makes an effort to encompass a wider sample, with only 21 cases of high levels of statistical significance, and a satisfactory representativeness of all possible subsystems cannot be claimed. Missing data, furthermore, undoubtedly precluded consistent findings among the various multivariate techniques applied. But the need to operationalize more of the variables deemed salient by Rosecrance and Waltz is the most glaring task remaining in order to achieve a richer body of tested propositions, particularly since the key independent variable—polarity—is less precisely measured than the most significant dependent variables.

With such caveats in mind, one might expand the main finding of a negative relation between system stability and number of power centers to congruent situations. Within a domestic political system, for example, it does not seem surprising to expect that strong centralized rule guarantees domestic stability much more than the presence of sectionalism or separatist groups. Supranational integration and national unification, similarly, are achieved more peacefully and effectively if engineered by single strong core states rather than by rival states or achieved in an egalitarian manner.[36] The tyrannical aspects of consolidated rule in monoelite systems, however desirable for limiting levels of violence, are not easily mitigated through institutional channels and by formal regulators, however. We are therefore confronted with a Hobson's choice between the pacific virtues of strong authority and the prospect of the evils of license in political systems, whether intranational or international, which permit more liberty to their constituent parts.

## DEFINITIONAL APPENDIX

1. *Number of poles.* Sum of significant military power centers, such as unaligned major powers, rival blocs or alliances. (Rosecrance)
2. *Unipolarity.* A subsystem containing one pole with a plurality in military powers without significant opposition.
3. *Bipolarity.* A subsystem with two main military power centers.
4. *Tripolarity.* A subsystem with three main military power centers.
5. *Multipolarity.* A subsystem with four or more military power centers, usually unaligned major powers.
6. *Availability of resources.* The presesce of neutral powers, that is, entities not aligned with the main poles; or, the existence of areas ripe for military expansion in view of their unguarded, power-vacuum status (antonym: unavailability). (Rosecrance)
7. *Tightness of poles.* Lack of neutral powers mediating significantly between poles (antonym: looseness). (Kaplan)
8. *Strength of regulator.* Systemic institutional device or practice with an ability to check aggression from one or more poles, such as balance of

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[36] Amitai Etzioni, *Political Unification* (New York: Holt, Rinehart, Winston, 1965).

power, alliance system, concert of major powers, intergovernmental organization. (Rosecrance)

9. *Number of members.* Sum of entities with military viability that interact more or less continuously within a region in a manner relevant to political-military goal attainment, adaptation, pattern maintenance, and integration.
10. *Number of major powers.* Sum of members that can be totally defeated in battle by no other single power, but instead by a combination of members (usually including another major power).
11. *Number of major and middle powers.* Sum of major powers plus members that can be defeated militarily only by single major powers. (NB. A *minor power* is the residual category for the weakest entity of a subsystem.)
12. *Number of satellites.* Sum of members that remain internally autonomous but have foreign policy decisions made by an outside power.
13. *Fully sovereign members/members.* Percentage of members retaining sovereign status throughout an entire subsystem era, excluding satellite states and (either abortive or successful) subnational entities in revolt.
14. *Members/middle and major powers.* See definitions 9 and 11.
15. *Major powers/major and middle powers.* See definitions 9 and 11.
16. *Number of alliances.* Sum of nonaggression pacts, defensive pacts, and ententes between entities. (Singer and Small)
17. *Aligned countries/members.* Percentage of sovereign members of a subsystem entering at least one alliance ("sovereign" at least for one year in the era).
18. *Alliance partners/alliances.* Mean number of countries in all alliances of a subsystem.
19. *Alliance partners/alliances/members.* See definitions 17, 18.
20. *Number of alliances between major and minor powers.* Sum of alliances with major powers and minor powers as members.
21. *Minor powers allied to major powers/minor powers.* Percentage of minor powers allied with at least one major power.
22. *Partners in alliances including minor and major powers/all such alliances/minor powers.* Mean number of members in alliances with at least one major and one minor power.
23. *Partners in alliances including both minor and major powers/all such alliances/minor powers.* See definitions 11 and 22.
24. *Number of alliances exclusively between major powers.* Sum of alliances contracted between major powers lacking either middle or minor powers as partners.
25. *Alliances exclusively between major powers/alliances including major powers.* Percentage of alliances containing at least one major power that have neither middle nor minor powers as partners.
26. *Partners in exclusive major power alliances/all such alliances.* Mean number of members in alliances lacking middle or minor powers as partners.
27. *Partners in exclusive major power alliances/all such alliances/major powers.* See definitions 10 and 26.
28. *Alliances starting in earlier era or ending in later era/alliances.* Percentage of alliances either contracted in a previous subsystem era or terminating in a later subsystem era.
29. *Alliances including both major and minor powers starting in earlier era or ending in later era/alliances including major and minor powers.* See definitions 10, 11, 20, 28.
30. *Alliances exclusively between major powers starting in earlier era or ending in later era/alliances including major and minor powers.* See definitions 10, 24, 28.
31. *Extraregion alliances/all alliances.* Percentage of alliances contracted between at least one member of a subsystem region and at least one nonmember of that subsystem region.
32. *Extraregion alliances including major powers/all alliances.* See definitions 10, 20, 31.
33. *Extraregion alliances including minor powers/all alliances including minor powers.* See definitions 11, 24, 31.
34. *Number of wars.* Sum of fatal quarrels involving more than the loss of 314 lives. (Richardson)
35. *Members at war/members.* Percentage of members fighting in at least one war.
36. *Participants in wars/members at war.* Sum of all belligerent participants in all wars divided by the sum of members fighting at least one war.
37. *Participants in wars/wars.* Mean number of members fighting per war.
38. *Durations of wars/wars.* Mean number of years per war (rounded up).
39. *Years with new wars/years in era.* Percentage of years in a subsystem era containing an outbreak of at least one new war.
40. *Number of wars with major powers.* See definitions 10, 34.
41. *Wars with major powers/major powers.* See definitions 10, 35.
42. *Participants in wars with major powers/all such wars.* See definitions 10, 37.
43. *Durations of wars with major powers/all such wars.* See definitions 10, 38.
44. *Years with new wars with major powers/years in era.* See definitions 10, 39.
45. *Wars resulting in destabilization/wars.* Percent-

age of wars won by members resulting in a change in ante bellum conditions, such as redrawing of boundaries or transfer of sovereign control over a population, favoring the aggressor.

46. *Wars with major powers resulting in destabilization/wars with major powers.* See definitions 10, 45.
47. *War deaths/wars.* Mean battle-related casualties for all wars fought in subsystem era.
48. *Wars starting in earlier era or ending in later era/wars.* See definitions 28, 34.
49. *Wars with major powers starting in earlier era or ending in later era/wars with major powers in era.* See definitions 10, 29, 34.
50. *Extraregion wars/all wars.* See definitions 31, 34.
51. *Extraregion wars with major powers/all wars with major powers.* See definitions 10, 32, 34.
52. *Level of disturbance.* Extent of international conflict challenging responses by the regulator to preserve an existing power distribution (Rosecrance).
53. *Number of wars.* Sum of instances of military combat between states involving formal declarations of war. (Wright)

54–63. Same conceptual variables and definitions as variables 34–44.

64–68. Same conceptual variables and definitions as variables 47–51.

# DIRECT LEGISLATION: SOME IMPLICATIONS OF OPEN HOUSING REFERENDA

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Any middle-aged member of the political science guild in a retrospective mood might ponder a question: "What ever happened to direct democracy?" In our halcyon student days the textbooks discussed the direct democracy trinity—initiative, referendum, and recall—described their mechanics and variations, explained their origin in the Progressive Era, told us that the United States, Australia, and Switzerland were leading practitioners of direct democracy, cited a few eccentric referenda, gave the standard pro and con arguments, and essayed some judgments of the relative merits of direct and representative democracy. Latter day collegians may pass through the portals innocent of the existence of the institutions of direct government. Half of the American government texts never mention the subject; the others allocate a paragraph or a page for a casual mention or a barebones explanation of the mechanics.[1]

A similar trend has occurred in the literature. Before 1921, every volume of this REVIEW had items on the referendum, five in one volume. Subsequently there have been only seven articles, all but two prior to World War II.[2] "The Initiative and Referendum in Graustark" has ceased to be a fashionable dissertation topic, only four in the last thirty years.[3] All but two of the published monographs antedate World War II.[4]

The diligent students of voting behavior have neglected referenda—perhaps one does not plough a field which one presumes already has eroded away. However some sociologists, in quest for indicia of alienation and anomie, have stumbled on to some referenda.[5] Community power studies have made limited use of referenda data.[6] A handful of studies have effectively employed referenda data for various purposes. Wilson and Banfield calculated the ethnic and income correlates of bond issue voting as indices of public—and private—regardingness.[7] Referenda data have been used to diagnose the nonsuccess of urban renewal and metropolitan government schemes.[8] Survey research has been employed to ascertain voting behavior in four case studies, three of open housing referenda, and a bond election in one community.[9] Scant effort has been made to use survey methods to illuminate the dynamics of referenda, to ascertain the policy consequences of direct legislation, or to appraise its efficacy as a decision-making instrument.

A casual explanation for the demise of direct democracy without the courtesy of an obituary

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* The author is indebted to Mrs. Duane Stranahan and the Bowling Green University Foundation for generous financial assistance.

[1] Of a dozen current American government taxes examined, four allocate a page to "direct democracy," three a paragraph or two, and five never mention the topic. State and local texts usually give it more attention.

[2] Listed in the *Cumulative Index of the American Political Science Review, 1908–1963,* p. 87.

[3] Unpublished doctoral dissertations: Anson Van Eaton, *The Initiative and Referendum in Missouri* (Missouri, 1955), Wallace H. Best, *Initiative and Referendum Politics in California* (So. Calif., 1956), Walter Rosenbaum, *Legislative Participation in California* (Princeton, 1964), Jack Goldsmith, *The Role of the Initiative as a Tactic of Interest Group Politics in California,* (UCLA, 1966).

[4] Joseph G. LaPalombara, *A Study of the Initiative and Referendum in Oregon* (Corvallis, 1950), Alvin Boskoff and Harmon Zeigler, *Voting Patterns in a Local Election* (Philadelphia, 1964).

[5] Thomas A. F. Plaut, "Analysis of Voting Behavior in a Flouridation Referendum," *Public Opinion Quarterly,* 23 (1959), 213–222; William Gamson, "The Flouridation Dialogue," *Public Opinion Quarterly,* 25 (1961), 526–537; John Horton and Wayne Thompson, "Powerlessness and Political Negativism," *American Journal of Sociology,* 67 (1962), 485–493; E. L. Dill and J. C. Ridley, "Status, Anomie, Political Alienation and Political Participation," *American Journal of Sociology,* 68 (1962), 205–213; Clarence Stone, "Local Referendums; An Alternative to the Alienated Voter Model," *Public Opinion Quarterly,* 29 (1965), 213–22.

[6] Robert Agger et al, *The Rulers and the Ruled* (New York, 1964), Robert V. Presthus, *Men at the Top* (New York, 1964), Oliver Williams and Charles Adrian, *Four Cities: A Study of Comparative Policy-Making* (Philadelphia, 1963).

[7] James Q. Wilson and Edward C. Banfield, "Public Regardingness as a Value in Voting Behavior," this REVIEW, 58 (1964), 876–887.

[8] Richard Watson and John Romani, "Metropolitan Government in Cleveland," *Midwest Journal of Political Science,* 5 (1961), 365–390; John M. Ducey, *Who Killed the Urban Renewal Bond Issue?* (Loyala University Center for Research in Urban Government, 1966).

[9] Boskoff and Zeigler, *op. cit.* The open housing studies are cited in footnote 16.

might run something like this. In contrast to the opening years of this century when life was simple and democratic feeling ran high, bubbling with naivete and excessive optimism, in the latter half of the century direct democracy is anachronistic for a polity of two hundred million, whose technical policy questions far exceed the ken of everyman and require a minute division of labor and responsibility, with decision-making assigned to elites, legislative and bureaucratic, with specific competencies. Direct democracy has no place in the age of organization. Although no one noticed, it necessarily went out with the horse and buggy, and the Madisons and Burkes who feared the excesses of democracy may rest in peace.

The presumption that direct democracy is of negligible significance seems exaggerated. Every year I am summoned to be a legislator four, six, or eight times, enacting or rejecting property tax levies, bond issues, nonproperty taxes, public works projects, redistricting the legislature, revising school districts, and assorted amendments of city charter and state constitution. In a recent year there were 1,846 referenda in Ohio.[10] Although direct democracy is almost nonexistent in some states, the national volume of referenda may be ten to fifteen thousand annually.[11]

Are referenda inconsequential despite the volume—merely perfunctory ratification of trivial matters? Indubitably many are trivial, such as minor technical amendments of a state constitution in a familiar contextual syndrome: proposed by a virtually unanimous legislature, sponsored by a few prestigious organizations, and without organized opposition. But other constitutional referenda are important and contested vigorously. The electorate's decisions are hardly trivial when it simultaneously rejects a legislative apportionment tailored for partisan advantage by a governor and his cohorts and their proposal to establish a state bond commission authorized to finance any state or local function (and leave the bills for successors).[12] Surely school boards and administrators do not view referenda as empty routine. Of six controversies in Chicago which Banfield selected as the most important in a recent period, the referendum was a factor in three; in each instance proponents of plans chose not to risk a referendum.[13] A legislature generously offers local units an assortment of "permissive taxes," but permission must also be secured from the local electorate, and the new "authority" remains dormant.[14] The presence or absence of the referendum has been crucial in two areas of public policy: fluoridation and open housing.

Not only is direct democracy slighted by the textbooks, but when mentioned the treatment is likely to be misleading. Generally the brief reference is exclusively to state level I and R, which also has been the focus of most of the literature. Although statewide referenda are frequent in a few states, notoriously California and Louisiana, the national average of state referenda is 70 in odd years and 300 in even years, about two per cent of the volume of local referenda.[15] Consistent with grass roots folklore, it is at the local level that direct democracy flourishes and affords abundant opportunities for study.

### I. OPEN HOUSING REFERENDA

Between 1963 and 1968, ten cities and the state of California conducted open housing referenda. All were initiated by opponents who utilized the referendum provisions of city charters or state constitutions in an effort to cancel open housing legislation by vote of the sovereign electorate. In some communities the petition drive was a project of John Birch Society activists. The strategy of nullifying the public policy set by the processes of representative democracy by invoking the machinery of direct democracy worked consistently until 1968. The California statute received only 35% support from the electorate, city ordinances from 25% to 47%. Indeed, until the surprising victory of the Flint ordinance in February, 1968, by a paper-thin margin on a recount, it appeared that open occupancy could never win at the polls.

The voting patterns have been investigated for three of them. The California "Proposition 14" in 1964 and the previous one in Berkeley were analyzed by matching census data with vote tallies by wards and secondary analysis of limited data from a commercial poll.[16] The Toledo referendum, September, 1967, was examined by a city-wide sample of 456 (usable) interviews conducted during the week preceding the referendum and interviews of protagonists.[17]

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[10] Ohio Election Statistics, 1957–58.

[11] A national sample of school referenda reports that the sample of 583 districts had 2,630 referenda in the decade, 1948–1959, which produces an annual national average of 13,000 school referenda for that period. Subsequent consolidation of districts has reduced the number but not the significance of school referenda. A. F. Carter and W. G. Savard, *Influence of Voter Turnout on School Bond and Tax Elections* (Office of Education, 1961), p. 10.

[12] The substance and fate of Ohio State Issues One and Two, May, 1967.

[13] Edward C. Banfield, *Political Influence* (New York, 1961).

[14] Local officialdom is not grateful for the "permissive taxes" recently authorized by the Ohio General Assembly.

[15] Tabulated by Hugh A. Bone, "Easier to Change," *National Civic Review*, 57 (1968), 120.

The Toledo survey explored dynamics of voting behavior in referenda. Some of the specific objectives were to measure the degree of interest and involvement among categories of the electorate; to ascertain the electorate's sources of information, advice, and influence; to measure the volume of communication and the voter's evaluation of information sources; to determine the effect of the contemporaneous riots and the amount of "white backlash"; to compare the non-voters with the minority which voted; and to correlate voting with attitudes and socio-economic variables.

The studies of the Berkeley and California referenda focus on voting correlates and description of the struggle. Here our concern is comparison of voting behavior in referenda and national elections and the implications of the open housing referenda for the theory of direct legislation, particularly its efficacy as a decision making process. The focus can be indicated by a few hypotheses: (1) Campaigns in open housing referenda are unlikely to result in much conversion, because we know that presidential campaigns principally reinforce beliefs and activate predispositions; in an open housing vote racial attitudes substitute for party identification as potent predispositions. (2) The existence of such strong predispositions will make ineffective the efforts of community elites to influence the outcome. (3) For the same reason recent riots will have little impact on the outcome. (4) The paramount determinant of turnout in referenda is the significance of the concurrent election (if any) rather than the significance of the issue because of the low salience of local politics. (5) For that reason and because most referenda are nonpartisan, turnout at local referenda usually will be low, a contradiction of a foundation premise of direct democracy. (6) The rationale of direct legislation as pure democracy also is clouded by the likelihood that those who do vote are somewhat unrepresentative of the populace. Some unrepresentativeness is almost a certainty, because we know that elections normally have some class and racial bias. That factor is likely to make the referendum figures an inaccurate, and possibly even erroneous, reading of public opinion in the jurisdiction. (7) Therefore the accuracy of referenda as measures of opinion for a jurisdiction is maximal when referenda are scheduled concurrently with presidential elections, and have the greatest potential for distortion or miscarriage when scheduled alone or concurrently with an insignificant election. (8) Because of the low salience of politics, the efforts of referenda protagonists to communicate with the electorate in any large jurisdiction will not reach a substantial portion of the populace, even on such extraordinary issues as open housing. (9) Therefore the information level regarding the issue will be rather low for a large portion of the population of any large jurisdiction (and often in small ones), which is inconsistent with the civic man premise of direct democracy and militates against the efficacy of the referendum as a public policy decisional process. (10) But the actual voters in referenda will have distinctly more knowledge than non-voters, as per our knowledge of voters and nonvoters, which improves the rationality of decision making by referendum at the cost of violating its own ethic of universality and augmenting the bias. (11) In a referendum numerous voters actually do not fulfill the legislative role of seeking and weighing all the pertinent data and considerations in a diligent search for the public interest, but instead vote their private preferences.

## II. INTEREST, STAKE, AND PARTICIPATION

Voting research has demonstrated that electoral participation is a function of an individual's psychological involvement in politics and has documented abundantly the slight interest of most people in politics.[18] The turnout rates of local elections indicate that psychological involvement usually is less in local than national politics. Although voting rates for local referenda fluctuate considerably, turnout is rarely high and occasionally below 10%.[19] This contradiction of grass roots mythology is clarified by recent investigations of the determinants of voter psychological involvement. It is diminished for referenda by the lower visibility of the objects of local than national politics and by the absence of partisanship. Intensity of party preference is the strongest index of voting in national elections.[20] The nonpartisanship of most referenda affects turnout doubly by removing both the strongest internal stimulus and the external stimulus of party organizations. Also referenda may impose additional burdens on the voter by being scheduled as "special elections" or by their abstruseness.

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[16] T. W. Casstevens, *California's Rumford Act and Proposition 14 and the Defeat of Berkeley's Fair Housing Ordinance,* pamphlets published by Institute of Governmental Studies, Univ. of Calif.; Raymond E. Wolfinger and Fred I. Greenstein, "The Repeal of Fair Housing in California: An Analysis of Referendum Voting," this REVIEW, 62 (1968), 753–769.

[17] Two hundred eight clusters of three housing units were drawn systematically. A randomizing device was used to spot the cluster within each clock and another to select the interviewee. Interviews were completed in all but one cluster, two in most, with a completion rate of 76%, exclusive of vacancies and razed areas.

[18] Angus Campbell et al, *The American Voter* (New York, 1960), pp. 89–115.

[19] School fiscal issues comprise about three-fourths of all referenda. The mean turnout at

Open housing might be that rare specimen, a referendum which generates psychological involvement equal to or possibly in excess of presidential elections. The issue does not appear complex or remote to most people, intensive campaigns are waged, candidates and parties take positions, and the referendum poses starkly what is perhaps the most fundamental and emotionally charged issue of domestic policy today. The 1964 California referendum appears to support that hypothesis; the turnout was 85% of the registered voters, distinctly higher than other referenda on the ballot, and only 2% below the presidential vote. However the Toledo referendum, a better test because of its separation from a presidential election, furnishes evidence to the contrary.

The paramount determinant of turnout for a referendum is scheduling—the significance of the concurrent election. Open housing indubitably generates more involvement than local politics normally. It established a city primary turnout record in Toledo, but 64% of the adult population did not go to the polls and 8% of those voting passed up the proposition. Furthermore, the turnout at the subsequent (nonpartisan) city election sans the housing issue was higher (44%), and much higher at the 1968 presidential election (80%).

TABLE 1. DEGREE OF INTEREST

| | | | |
|---|---|---|---|
| | *Great* | *Little* | *None* |
| Toledo FHO Referendum | 46% | 37% | 17% |
| SRC National Sample | *Very much* | *Somewhat* | *Not much* |
| 1956 Presidential Campaign* | 30% | 39% | 31% |

* Calculated from data in *The American Voter*, 103.

The relative inability of referenda to move people to the polls is underscored by comparing the professed level of interest in the Toledo referendum with an SRC national sample's interest in presidential elections. Employing the same yardstick, the professed interest level of the Toledo sample was higher. Here is a pronounced contrast of electoral behavior in national elections and local referenda: corresponding levels of professed interest may not produce comparable voting rates.

Interest and other dimensions of psychological involvement, such as political efficacy, do exert comparable effects on individual decisions to vote or abstain in local referenda and national elections. The pattern of association is the same in both types of elections (Table 2), but the starting plateau is much higher for presidential elections, which attract even a majority of the persons at the bottom of the interest and political efficacy scales. Local referenda, even on open housing, do not approach the magnetism of presidential elections.

The Toledo housing referendum was no exception to the proposition that politics are not salient for the populace. The controversy was rather an

TABLE 2. PSYCHOLOGICAL INVOLVEMENT AND TURNOUT

| | Percent Voting | | | Percent Voting | |
|---|---|---|---|---|---|
| Degree of Interest | Toledo FHO Ref. | 1956 Pres. Election* | Sense of Political Efficacy | Toledo FHO Ref. | 1956 Pres. Election* |
| Low | 19 | 58 | Very low | 27 | 52 |
| | | | Low | 37 | 60 |
| Medium | 30 | 72 | Medium | — | 75 |
| High | 45 | 87 | High | 35 | 84 |
| | | | Very high | 41 | 91 |

* SRC national sample. *The American Voter*, 103, 105.

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school fiscal referenda was 36.3% of eligible voters in the 1948–1959 period. Carter and Savard, *op. cit.*, 12. Turnout rates on four fiscal referenda in upstate New York towns ranged from 7% to 45%. R. V. Presthus, *Men at the Top* (New York, 1964), 259–261.

[20] Campbell et al, *loc cit.*

TABLE 3. PERSONAL STAKE IN FHO BY RACE

"Do you think the outcome of the election will affect you personally . . . a great deal, only slightly, or not at all?"

| | Whites | Negroes | Total |
|---|---|---|---|
| Great deal | 14% | 25% | 16% |
| Slightly | 26 | 17 | 24 |
| None | 50 | 44 | 48 |
| Don't know | 10 | 13 | 11 |

academic matter for many respondents; only 40% perceived its outcome as affecting them personally, and only 16% felt a great personal stake in it. More of the Negroes perceived a personal stake, but less than half thought the outcome would affect them personally. One reason for the apathy of Negroes was pessimism. "A lot of Negroes feel it won't pass, and will have little effect if passed."

The Toledo referendum also adhered to another pattern of electoral behavior: lower participation by Negroes. That issue might be expected to produce an exception to the pattern, but it did not. The election day impression was confirmed by an examination of election records which disclosed that 37% of the whites in the sample actually voted but only 28% of the Negroes. Here again the principal determinant is the significance of the concurrent election. There likely was less Negro voting gap in the California referendum concurrent with a presidential election which drew 85% of the registered voters to the polls.[21]

Applying the sample voting ratios to (amazingly accurate) population estimates enables one to estimate the number of Negro and white Voters in the Toledo referendum and to ascertain the responses to the issue by white Voters. Approximately 9,000 votes were cast for open housing by Negroes and 14,000 by whites. It follows that only one-fifth of the white Voters supported the ordinance; the pattern of white voting was 20% for, 70% against, and 10% abstention.

### III. THE NONVOTERS

Low participation rates not only are at variance with the premise of direct democracy that the people wish to be legislators, but also vitiates somewhat its claim to be a more authentic expression of the "general will" than the representative body whose decision is being challenged. Therefore, how representative of the views of the populace was the active minority, one-third of Toledo adults, that made the open housing decision? The vote could be unrepresentative in either direction depending upon which side achieved the greater mobilization. The larger volume of nonvoting shows that mobilization was quite incomplete on both sides, but all the data indicate that mobilization of the potential pro vote was less effective.

Table 5 indicates that the nonvoters, as an aggregate, were distinctly more favorable to open housing than the voters. A greater portion of nonvoters was pro and a smaller portion was hostile, in part because Negroes were a larger fraction of the nonvoters than of the voters. The data indicate that among whites opposition was a more intense motivation to get to the polls than a mildly favorable view of the ordinance. The "antis" voted, which corresponds to the findings of some studies of fluoridation referenda[22] and to our folklore that voting "agin" is a stronger incentive than voting for.

The unrepresentativeness of the social compo-

TABLE 4. PARTICIPATION BY RACE

| | *Est. 1967 Population | × | **% Adult | = | Estimated Adults | × | % Voters in Sample | = | Voters by Race |
|---|---|---|---|---|---|---|---|---|---|
| White | 329,986 | | 61.2 | | 201,952 | | 36.9 | | 74,520 |
| Negro | 62,567 | | 53.2 | | 33,285 | | 28.4 | | 9,453 |
| | 392,553 | | | | | | Calculated | | 83,973 |
| | | | | | | | Actual | | 84,074 |

* City Planning Commission.
** 1960 Census.

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[21] The Berkeley turnout of registered voters was slightly higher in census tracts with over half Negro residents than in other tracts, but the data furnish no measure of how many Negroes were unregistered. T. W. Casstevens, *op cit.,* 87. Most Negroes were not registered for the Toledo referendum.

[22] See items in footnote 5.

TABLE 5. POSITION ON FHO: OFFICIAL VOTE AND SAMPLE*

| | Official Vote | % | 161 Voters | 294 Nonvoters |
|---|---|---|---|---|
| For FHO | 22,827 | 27.2 | 31.7 | 39.1 |
| Against FHO | 54,518 | 64.8 | 63.4 | 51.0 |
| Not Voting | 6,728 | 8.0 | 5.0 | 9.9 |
| | 84,074 | 100.0 | 100.0 | 100.0 |

* Position stated in interview preceding referendum. $.05 > p > .02$.

TABLE 6. CLASS AND RACIAL COMPOSITION

| | Voters | Nonvoters |
|---|---|---|
| LOW edn., income, voc. | 33% | 49% |
| HIGH edn., income, voc. | 19% | 10% |
| Mean SES index | 3.5 | 2.7 |
| Negro | 12% | 16% |

sition of legislatures is manifest; as Long says, an election is an oligarchical device.[23] Comparison of the voters and nonvoters confirms that direct democracy also has distinct social bias. Because of low turnouts, local referenda are likely to have more class bias than major elections.

In this instance the unrepresentativeness of the effective electorate produced only a distorted reading of public opinion rather than a miscarriage; but referenda have such a potential when the outcome is close and turnout is low. Those conditions appear mutually exclusive to some extent; the closer the outcome, the less nonvoting should occur. These data do not deny such a relationship, but the significance of the concurrent election is a more potent determinant of turnout. It follows that the miscarriage potential of referenda is maximal when scheduled alone or with an insignificant election and minimal when concurrent with a presidential election.

### IV. EFFECTS OF THE CAMPAIGNS

What are the effects of the heated, intensive campaigns in open housing referenda? Do they merely neutralize each other, is there any substantial amount of conversion, or is voting a reflex of preexisting attitudes? For presidential elections, the Erie County and subsequent surveys have provided some of the most solid propositions of voting behavior. Presidential campaigns reinforce beliefs and loyalties, active predispositions, and convert some people; there is much reinforcement and activation but very little conversion.[24] A leader of the Right to Sell Committee agreed that its campaign activated rather than converted, but leaders of the Fair Housing committee believed that their campaign achieved quite a bit of conversion. Our interviews confirmed that there was some conversion, but the following data suggest that the volume was modest.

At least half of the voters in the sample had made up their minds before the campaign began. A fourth said they were undecided, but some merely did not wish to reveal their positions. Only a quarter felt they had been influenced by the campaign, which allowed small room for conversion. Eighty-eight respondents indicated the direction of the influence. The limited evidence indicates that the pro-FHO campaign achieved some net gain among the populace, but mostly among non-voters. Among the voters the anti-FHO campaign's activation effort apparently offset any pro-FHO conversions.

The data indicate that most of the voters had made their decisions prior to the campaign, only a fourth were influenced by it, there was little conversion, and hence the principal effects were reinforcement and activation. These inferences are corroborated by the Berkeley and California case studies. In California an upsurge of support for Proposition 14 occurred during the last weeks of the campaign. Wolfinger and Greenstein's analysis shows that it was a matter of increased understanding of the ballot situation, i.e., that yes was a no vote on open housing. The campaign "had the consequence of making clear

TABLE 7. IMPACT OF CAMPAIGN ON VOTERS (N = 161)

| | *Decided* | *Undecided* | *NA* |
|---|---|---|---|
| "Have you decided how to vote on it?" | 75% | 24% | 1% |
| "Did you make up your mind during the past month, or earlier?" | *Past Month* | *Earlier* | *Undecided & NA* |
| | 19% | 50% | 31% |
| "Has your viewpoint been influenced by what you read or hear in the campaign?" | *Yes* | *No* | *DK & NA* |
| | 26% | 71% | 3% |

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[23] Norton Long, "Bureaucracy and Constitutionalism," 56 (1952), this REVIEW, 56 (1952), 810.

[24] Paul Lazarsfeld et al, *The People's Choice* (New York, 1944), chs. 8–10.

TABLE 8. DIRECTION OF INFLUENCE

"Did that make you more for the ordinance or more against it?"

| | Voters | Nonvoters | Total |
|---|---|---|---|
| More For | 11% | 11% | 11% |
| More Against | 14 | 5 | 8 |
| No Effect | 75 | 84 | 81 |

to voters how to express their attitudes. It did not lead them to change their attitudes."[25] In the Berkeley campaign "the major effect was to activate and rationalize the predispositions of the voters."[26]

The SRC staff's field theory explanation of those effects in presidential elections is appropriate here. In opening housing referenda also "the psychological forces guiding behavior arise before the campaign opens" and three-fourths of the electorate have made their decisions beforehand.[27] The forces that generate the strong predispositions of voters in presidential elections usually are much weaker or absent in referenda, although party identification was significant in the Berkeley and California ones, but for an open housing referendum racial attitudes furnish a functional equivalent of even greater potency. Referenda on other policies may afford more scope for campaign impact on outcomes.

## V. EFFECTS OF RIOTS

If campaigns have slight effect on outcomes of open housing referenda aside from clarification of the ballot, what about "white backlash" from riots? The 1967 riots in neighboring Detroit and elsewhere coincided with the Toledo referendum. One might surmise that the riots were influential, because the yes vote in Toledo was only 30% compared to 34.6% in California and below that of all but one of the other cities. However, the lower Toledo vote might be a function of differences in the scope of the various propositions and/or differences in turnout rates. The latter furnish a prima facie explanation in light of the differences in the attitudes of the voters and nonvoters. Table 5 indicates that had the Toledo decision coincided with a presidential election the yes vote would have been 34.5%. It follows that backlash from the riots could have changed a substantial number of votes only if the Toledo populace prior to the riots was distinctly more sympathetic to open housing than the populace of California. I am unaware of any evidence to sustain that implausible assumption; these data indicate that the riots did not change many votes.

The riots did affect the thinking of several respondents and it was decisive for a few, but the riot impact was not unidirectional. A fourth of the respondents said the riots had influenced them negatively, but a sixth was influenced positively, reasoning that the riots were evidence of the need for open housing.

The responses to the attitudinal items of Table 9 are not indicative of strong white backlash. A large portion of the public attributed the riots to agitators rather than to Negroes collectively. Three-fourths did not anticipate that an open housing law would generate disorder, and only a sixth assented to the proposition that the riots justified denying Negroes "equal housing rights." (The question did not require respondents to equate "equal" and "open" and evidently "separate but equal" remains valid in many minds.) The responses to the public policy question of Table 10 also do not suggest much white backlash sentiment.

## VI. PUBLIC INTEREST OR PRIVATE PREFERENCE?

The rationale of direct legislation rests on the populist premises that the plebiscite is the purest form of democracy and that the public interest is the will of the majority of those voting. Critics of direct democracy, perhaps not wishing to appear undemocratic, usually do not challenge the democratic theme, which the foregoing

TABLE 9. RIOTS—ATTITUDES

| | SA | A | D | SD | DK |
|---|---|---|---|---|---|
| "The recent riots show that Negroes should not have equal housing rights." | 2% | 15% | 49% | 17% | 16% |
| "If it were not for a few firebrand agitators, we would not have racial unrest and disorders." | 15% | 52% | 19% | 4% | 10% |
| "A Fair Housing Ordinance will cause racial trouble in Toledo." | 4% | 23% | 42% | 8% | 23% |

Strongly agree to strongly disagree, DK—don't know.

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[25] Wolfinger and Greenstein, *op. cit.*, 758.

[26] Casstevens, *op. cit.*, 79.

[27] Campbell et al, *op cit.*, 76.

data indicate is vulnerable, but instead focus their fire on its efficacy as a public policy decisional process, questioning the electorate's capability to legislate wisely because of allegedly inadequate knowledge. Pollock examined this issue by assaying the judgment over time of the Michigan electorate and concluded that it was at least as good as the legislature's.[28] However, some recent observations are less sanguine, specifically with respect to open housing referenda,[29] and there is no dearth of anecdotes of whimsical incidents, e.g., voting for a new fire department and simultaneously rejecting the implementing tax levy.[30]

Assaying the record of the electorate's judgments is an extraordinary subjective enterprise, but there may be an alternative approach. Do voters at referenda express their respective judgments of the public interest or their personal preferences? It would seem that when the electorate undertakes to second-guess the legislature it has the same responsibility to search for the public interest. In terms of Simon's efficiency decision-making model, $E_a = O_a/I_a$, a voter is obliged to determine whether the net benefits exceed the costs.[31] Simon's model is strikingly apropos to fiscal referenda, by far the most numerous species. In a tax levy "election," does the voter ponder in the booth whether the community benefits are commensurate with the cost, or does he weigh his personal benefit against the increment of his property tax?

If voters substitute in Simon's formula their personal values, a plebiscite is logically a sound decisional mechanism only if the majority of private preferences automatically equates with the public interest. That populist premise, also the controverted premise of much interest group literature and some defenses of the rationality of federal budgeting,[32] has face validity perhaps for a few referenda, e.g., to legalize colored margarine. Its validity for most referenda is not patent and is demonstrably false for open housing and fiscal issues, because the incidence of costs and benefits is so unevenly distributed. Indeed the disparity is extreme in school referenda, where the paramount beneficiaries are arbitrarily barred from voting and most of the voters incur direct, visible costs and no direct benefits. (Are plebiscites really pure democracy, even sans property qualifications for voting?) Open housing is another situation in which the disparity is pronounced; some persons incur benefits and no costs while others perceive themselves as incurring only costs.

TABLE 10. JUDGMENT OF FHO AS PUBLIC POLICY

"Do you think the F.H. Ordinance would be *good* or *bad* for Toledo?"

| | Good | Bad | Good & Bad | Little Effect | Don't Know |
|---|---|---|---|---|---|
| Sample | 44% | 26% | 1% | 4% | 25% |
| Voters | 40% | 30% | 2% | 6% | 23% |
| Nonvoters | 47% | 25% | 1% | 4% | 24% |

To investigate this possibility, the sample was asked, "How do you think the Ordinance would affect the city? Do you think it would be good for or bad for Toledo?" The distribution of the responses (Table 10), even those of the voters is conspicuously contradictory to the results of the referendum. Evidently many ballots were not public policy judgments but expressions of personal preference, that is, not judgments of the public interest but of private interest. (The data below on voter information are consistent with this analysis.) This phenomenon casts some shadow on the efficacy of the referendum as a policy process.

## VII. KNOWLEDGE

Unless one holds that the efficacy of the referendum as a policy making process is irrelevant either because the electorate's decision is ipso facto the public interest or "the people are entitled to make their own mistakes" and "deserve the government they get," the allegation of critics that direct democracy overtaxes the information of the electorate merits investigation. The overburdened-uninformed-voter argument is clinched by citation of the referenda volume in a few states (California averages twenty-two per general election[33]); hence an isolated referendum is not an acid test of voter knowledge. Indeed the Toledo case presents an optimal situation for electoral knowledge: a single, local referendum on an extraordinarily vital policy, bitterly contested and highly publicized.

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[28] James K. Pollock, *The Initiative and Referendum in Michigan* (Ann Arbor, 1940), pp. 66–70.

[29] See Wolfinger and Greenstein, *op. cit.*, 768–769.

[30] There are some authentic instances in the Ohio archives.

[31] Herbert Simon, *Administrative Behavior* (New York, 1947), ch. 9.

[32] Eg., Aaron Wildavsky, *The Politics of the Budgetary Process* (Boston, 1964).

[33] Joseph P. Harris, *California Politics*, 4th ed. (San Francisco, 1967), p. 112. A recent study of voting patterns on California referenda by examination of absentee ballots furnishes data that reflect voter ignorance (only 53% voted on all propositions and the average abstention rate was 15% per proposition) and some confusion. On two antithetical propositions regarding a state lottery, only 47% voted consistently, 45% voted partially or completely contradictorily. John E. Mueller, "Reason and Caprice: Voting on Propositions" (ms, 1968).

Several survey questions solicited knowledge of the issue and the positions of organizations and candidates. Four-fifths of the sample understood the basic nature of the ordinance, but few knew that it had been enacted by unanimous vote of the council. Only a fourth were aware of the existence of the (less comprehensive) Ohio open housing statute; consequently few people had occasion to wonder, "Is the ordinance superfluous?"

A major gambit of the Fair Housing Committee was solicitation of endorsements by community organizations and publication of the list contrasting its forty endorsements with none for the Right to Sell Committee. That publicity preceded and coincided with the dates of the survey; nevertheless half of the sample could name no organizations which supported or opposed the FHO.

Which organizations were the most visible to the electorate? Followup questions regarding the positions of eight major organizations revealed slightly more knowledge than replies to the free answer question. Most people knew or reasoned that the Council of Churches supported it, indicating that the intensive activity of church people was successful to that extent. If people knew anything about the FHO campaign, it was that the churches were for it.

Half of the sample knew or reasoned that the ordinance was supported by the Board of Community Relations (city civil rights agency) and the AFL-CIO Council. The endorsements of the Council and prominent union officials were heavily publicized and the Council's election flyer to unionists carried its endorsed slate on one side and an appeal for the ordinance on the other side. Rarely have union officials taken as prominent a stand for a cause beyond the scope of union direct interest, but their voices were not heard by half of the citizenry nor by most union members.[34]

An interesting aspect of Table 11 is that more respondents identified the Democratic Party as for the ordinance than the Republican Party, although the Republican City Committee's endorsement was reported in the press only a few days before the survey, the ordinance was sponsored by the Republican mayor, and the Democratic mayoralty candidate simultaneously was finding defects in the ordinance and obviously courting the votes of opponents. Evidently very few people actually "knew" that the local parties had endorsed FHO and instead more people inferred Democratic Party endorsement from their impressions of the stance on civil rights of the parties nationally. It also is evident that this instance of bipartisanship had scant weight on the outcome.

The results of the primary and referendum were astonishingly incongruent. Only one anti-FHO candidate survived while the mayor and councilmen who had enacted the ordinance won handily. One explanation of the anomalies is that the electorate had an unusual situation: electors could have their cake and eat it too. The survey data furnish another explanation. The electorate did not associate the issue with candidates, because most of the public did not know the positions of the candidates. Three-fourths of the sample could not name a single

TABLE 11. POSITIONS OF ORGANIZATIONS-RECOGNITION

"Do you know the positions of any of these organizations, are they neutral, for it, or against it?"

| | Actual Position | Pct. Correct Answers | | |
|---|---|---|---|---|
| | | Voters | Nonvoters | Total |
| Toledo Employers Association | For | 39 | 27 | 31 |
| Toledo AFL-CIO | For | 66 | 44 | 52 |
| Board of Community Relations | For | 68 | 44 | 52 |
| Toledo Council of Churches | For | 83 | 64 | 71 |
| Republican Party | For | 40 | 22 | 29 |
| Democratic Party | For | 48 | 34 | 39 |
| The Toledo Blade | Neutral* | 11 | 7 | 8 |
| League of Women Voters | For | 37 | 22 | 27 |

* Blade endorsement (weak) came after the survey.

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[34] Only 44% of the membership knew of the UAW union's endorsement despite 15 articles in the union organ. Schley R. Lyons, "Labor in City Politics: The Case of the Toledo United Auto Workers," (paper at 1968 meeting of Ohio Association of Political Scientists).

council candidate pro-FHO and nine-tenths could name no anti-candidate.[35] When queried about the stance of the three mayoralty candidates and six Council candidates, the incumbent mayor who vigorously championed FHO was the only one whose position was visible to the public.[36]

Manifestly most of the public paid little attention to the campaign. In the final week the electorate's knowledge of significant aspects of the issue, of the candidates, and their positions was meager. About all most adults knew was the existence of the referendum, the basic nature of an open housing ordinance, and that the ordinance was supported by churches and the mayor.

Did knowledge advance materially in the last four or five days? Perhaps, but the Erie County study suggests otherwise; at the peak of that presidential campaign "about half of the population ignored stories on the front page of their newspapers and speeches by the candidates" and persons with low exposure in the early part of the campaign continued to have low exposure throughout.[37]

Referenda require no qualification of two axioms of voting behavior: that "in the electorate as a whole the level of attention to politics is so low that what the public is exposed to must be highly visible—even stark—if it is to have an impact on opinion,"[38] and that voters generally are more informed than nonvoters. The knowledge level of voters in the sample was substantially higher than nonvoters on all measures of information, although one could hardly characterize the aggregate voters as well informed, when a majority did not know the positions on the issue of the political parties, the leading newspaper, and most candidates.

The logic of direct democracy predicates higher levels of knowledge than is required for representative democracy; homo politicus is its postulate. The foregoing data and a simultaneous study of Toledo's largest labor union[39] surely do not demonstrate that information levels are higher in referenda than other elections. The measurements of the Elmira study are too dissimilar for a good comparison, but the knowledge level in Elmira in that presidential election appears at least equal to this referendum.[40]

There are alternative "explanations" of the failure or refusal of many Toledo citizens to become well informed on the open housing issue (that is, to acquire information available in the mass media regarding the positions of organizations and council candidates and the existence of the state open housing law): in moralistic terms as dereliction of civic duty, in psychological terms as selective attention filtering out information, or in economic terms.

Downs' economic logic, that nonvoting and low information are rational behavior because the costs of acquiring information exceed the stakes of the election for most people, would seem less applicable to an open housing referendum whose consequences are more distinct and significant than most elections.[41] There are, however, elements in this situation that fit the economic theory: 60% of the populace did not view the outcome as affecting them personally, the issue had the appearance of simplicity rather than abstruseness, and the voting decision rested more on attitudes than facts; ergo expenditure of effort to acquire information would be uneconomical. If information-seeking was uneconomical here, would it be otherwise for any statewide or many local referenda? If the Downs' theory is a valid explanation of the rationality of inattention and passivity vis-a-vis elections, its implication for the efficacy of direct legislation is not supportive.

## VIII. COMMUNICATION

How much communication is generated by a referendum, what fraction of the populace observes references to the issue in mass media and reads handbills, how much is the issue discussed at meetings and informally, and do organization leaders succeed in communicating with the membership? The open housing issue should generate more communication than most referenda, therefore the measurements below perhaps approach the upper limits of citizen involvement in referenda campaigns.

Again benchmarks are missing; there are no closely comparable measurements of communication in referenda or election campaigns. We have some knowledge of aspects of election campaign communication: that exposure is distinctly a function of interest, education, age, and SES; that interested persons have high exposure

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[35] The electorate's ignorance was partially because the strategy of council candidates, except the anti-FHO ones, was to avoid the issue like a plague.

[36] There were parallels between the 1964 California election and the Toledo city election. The Republican mayor that championed FHO was defeated by the Democratic challenger who found technical flaws in the ordinance during the primary and campaigned valiantly against crime in the streets.

[37] Lazarsfeld et al, *op. cit.*, ch. 14.

[38] Campbell et al, *op. cit.*, 60.

[39] Lyons, *op. cit.*

[40] Bernard Berelson et al, *Voting* (Chicago, 1954), ch. 10.

[41] Anthony Downs, *An Economic Theory of Democracy* (New York, 1957), pp. 207–276.

TABLE 12. COMMUNICATION: CONVERSATION

(Pct. vertically)

| | Frequency | Voters | Nonvoters | Total |
|---|---|---|---|---|
| "How many times have you discussed the FHO with members of your *family?*" | None | 24 | 44 | 27 |
| | Few | 38 | 34 | 35 |
| | Several | 35 | 17 | 24 |
| | DK or NA | 3 | 5 | 4 |
| "How many *neighbors* have you talked to about it?" | None | 48 | 67 | 60 |
| | Few | 29 | 19 | 23 |
| | Several | 20 | 11 | 14 |
| | DK or NA | 3 | 3 | 4 |
| "How many *friends* have you talked to about it?" | None | 27 | 46 | 39 |
| | Few | 33 | 36 | 34 |
| | Several | 38 | 15 | 23 |
| | DK or NA | 2 | 3 | 3 |

to all the mass media and uninterested persons low; that party identification is somewhat associated with choice of media; and that conversation and personal influence remain important, as opinion leaders provide linkage between mass media and the less active segment of the population.[42]

Open housing should and did generate a lot of conversation but little organized discussion. Approximately three-fifths of the sample and three-fourths of the voters discussed it with relatives and friends; a hyper-active third of the voters discussed it "several times." There was distinctly less talk with neighbors. Half of the voters and 60% of the nonvoters professed not to know the attitudes of neighbors, but three-fourths of those voters who professed to know reported that their neighbors were opposed to the ordinance, which corresponds to the voting and indicates that the attitudes of neighbors are more consequential than the respondents rated their influence (below). The neighbors of nonvoters were less hostile to the ordinance, another indication that the effective electorate on September 12 was not an unselected sample.[43]

Organizations were channels of communication and (less) influence, because 40% of the sample, a slight majority of the voters, and a third of the nonvoters belonged to organizations which they knew had endorsed the ordinance. The relevant affiliations were church and union; for this issue, they and the ad hoc campaign committees were the important political organizations of the city. But communication between leaders and membership was incomplete, particularly in the unions. Despite endorsement by prominent union officials and the AFL-CIO Council, only a quarter of the unionists perceived endorsement by their union.

TABLE 13. COMMUNICATION: MEETINGS AND ORGANIZATIONS

Was the lower frequency of conversation with neighbors than friends and family a function of urban social distance or the nature of the subject?

(Pct. Vertically)

| | | Voters | Nonvoters |
|---|---|---|---|
| "Have you attended any meetings where it was discussed?" | Yes | 15 | 11 |
| | No | 84 | 85 |
| | DK or NA | 1 | 4 |
| "Place or type of meeting?" | Church | 7 | 4 |
| | Union | 1 | 0 |
| | Other | 4 | 1 |
| | Multiple | 1 | 0 |
| "Has any organization to which you belong taken a stand?" | Yes-For | 52 | 33 |
| | Yes-Against | 1 | 0 |
| | No or DK | 47 | 67 |
| "What is that organization?" | Church | 42 | 28 |
| | Union | 7 | 4 |
| | Other | 1 | 1 |
| | Multiple | 4 | 1 |
| | | Voters | Nonvoters |

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[42] Lazarsfeld et al, *op. cit.*, chs. 5. 14; Berelson et al, *op. cit.*, ch. 11.

[43] Responses to the query about the attitudes of neighbors were:

| | | % Voters | % Nonvoters |
|---|---|---|---|
| "Are most of your neighbors for the ordinance or against it?" | For | 12 | 11 |
| | Against | 35 | 26 |
| | Both | 3 | 2 |
| | DK | 50 | 61 |

TABLE 14. COMMUNICATION: NEWS MEDIA

| | | Voters | Nonvoters |
|---|---|---|---|
| "Have you seen any advertising for or against?" | Pro advertising | 51 | 37 |
| | Con advertising | 30 | 19 |
| | None | 40 | 54 |
| | DK or NA | 9 | 8 |
| "Have you heard any radio or TV programs or spot announcements?" | Pro programs | 66 | 53 |
| | Con programs | 42 | 36 |
| | None | 26 | 37 |
| | DK or NA | 6 | 8 |

Mass media and the churches were the principal informational sources.[44] Sixty percent of the sample heard some radio and TV programs or announcements, about 40% observed newspaper advertisements, and a third reported reading news accounts and feature stories. Meetings were an important but less frequent source. Thirteen percent of the sample reported attending meetings where the FHO was on the agenda—principally church groups. (That figure may be an understatement, because few respondents classified church services as "meetings.")

The sharpest aspect of these measurements of communication is the congruence with the Erie County study findings of the distinctly lower involvement of nonvoters.[45] The voters belonged to more organizations that endorsed FHO, attended more meetings where it was discussed, observed more references in the mass media, and talked about it much more than the nonvoters. However voting does not correlate fully with campaign exposure in a low turnout election; some of the voters were low on exposure and numerous persons who were aware of the FHO campaign did not vote. These data may indicate the ceiling of communication on a public policy issue in a large jurisdiction. Approximately 40% of Toledo adults were oblivious of all the propaganda issued by the Fair Housing and Right to Sell committees, the churches, unions, and anti-FHO candidates for council and mayor.

## IX. INFLUENCE OF ELITES AND THE PRESS

Are some sources of information and advice on referenda distinctly persuasive? "On a matter like the Fair Housing question, whose judgment would you respect and place confidence in? Would you be slightly influenced, greatly influenced, or not influenced at all by each of these?" Seven sources were enumerated: newspaper and TV editorials, the comments of friends and neighbors, and recommendations of the mayor, one's minister, or union. If the respondents were candid, there were no preeminent molders of opinion. There were only two deviations: a fifth of the unionists ascribed great influence to their union[46] and a fifth of the sample assigned great influence to their minister on this matter. Although respondents possibly underrated, because the forces of influence are subtle and people prefer the self-image of being an independent thinker, their denials of influence are congruent with the overwhelming defeat of the FHO in the face of its endorsement by the press and community elites.

Rarely are the political behaviors of elites and masses so different as in open housing decisions. The contrast was sharper in Toledo, where the elites maintained at least a facade of united endorsement of FHO, than in California where realtors, some taxpayer groups, and two statewide party organizations opposed it,[47] but the patterns were the same. The elites were more tolerant, other-regarding, and innovative; the masses were more conservative, less tolerant, and more self-regarding. This is consistent with the findings of analyses of referenda in two other policy areas, fluoridation and bond issues.[48]

The open housing plebiscites demonstrate the limitations of elite power. The Toledo story can be stated tersely, the elites were on one side and the votes on the other. The hypothesis of some community power students that elites are so much our decision makers that outcomes of referenda can be predicted by examination of the alignment and activity of elites manifestly does not hold for open housing referenda.[49] Nor does it hold for other important policy areas: school levies, state and local bond issues, city annexation, and metropolitan government schemes. In school tax levy "elections," elites characteristically are aligned on the pro side but a substantial portion are defeated.[50] How many "power elite" visions of civic monuments, "growth," and metropolitan government have foundered on the rock of rejection at the polls of "progress" by the intransigent masses? The processes of representative democracy often are more useful for elites and the referendum not infrequently is an insuperable barrier to elite goals.[51]

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[44] About 30% of the sample reported receiving handbills.

[45] Lazarsfeld et al, *op. cit.*, 45–49.

[46] Lyons' survey of the UAW, whose leaders plumped for FHO in the *Union Journal*, reports that although 44% of the membership were aware of the union's endorsement, only 29% favored FHO. But union influence was visible: 60% of Caucasians scoring high on a pro-union political orientation scale favored FHO but only 28% of the Lows favored it. Lyons, *op. cit.*

[47] T. W. Casstevens, *op. cit.*, 57–62.

[48] See citations in footnotes 5 and 7.

[49] Delbert C. Miller, "The Prediction of Issue Outcome in Decision-Making," *Proceedings of the Pacific Sociological Society*, 25 (1957), 117–197; Robert C. Hanson, "Predicting a Community Decision: A Test of the Miller-Form Theory," *American Sociological Review*, 24 (1959), 662–676.

[50] In Ohio from 1960 to 1968, 48% of school bond

Textbooks inform us that newspapers exert great influence in local referenda,[52] which may be correct, but is unsupported by the limited amount of research to date. Examining the absentee ballots of the 1964 election in Los Angeles, where the press was generous with advice, Mueller found "little effect on the vote on the propositions" by the newspapers.[53] Both Toledo newspapers endorsed the FHO, although the Blade hedged its reputation for influence in local elections by predicting defeat of the ordinance. The Blade editors understood the limitations of elite influence in referenda.[54]

## X. CONCLUSION

The variations in the votes of the eleven open housing referenda demonstrate that outcomes are affected by differences in the scope of the laws, in timing, and the nature of communities; but the data of the Berkeley, California, and Toledo referenda indicate that the vigorous campaigns have slight effect on the results. This parallel of presidential campaigns comes from the same source, the electorate's potent predispositions that are activated but rarely changed by a campaign. By extension, the opportunity for conversion in referenda campaigns is a function of the relevance of the issue to any strong predispositions in the electorate.

In Toledo the predispositions rooted in racial attitudes were so strong that the contemporaneous riots had slight effect on the outcome, and the efforts of community elites in a veritable phalanx were unsuccessful. A characteristic aspect of referenda is that some community elites campaign for a measure, often with no visible opposition from others. Their success is likely to relate to the presence or absence of strong predispositions and their congruency with the elite's objective.

The Toledo data display additional parallels to the findings of voting research of presidential elections. Referenda are no exception to the low salience of politics, which is such a communication barrier that in a large jurisdiction protagonists do not reach a substantial portion of the populace. Referenda voting, i.e., the decision to vote, is a function of the same dimensions of psychological involvement, such as interest and political efficacy, and voting has a similar association with class and race. Each of those consistencies has distinct significance for direct democracy theory: for the validity of its universalist ethic and its homo politicus premise, and for its efficacy as a public policy decisional process.

The most conspicuous divergences of the Toledo data are that the turnout plateau was far below that of a presidential election and, although the electorate's professed level of interest was equal to that of presidential campaigns, the turnout was less than half as much. That is not a baffling anomaly; it is the product of the lower salience of local politics and, in this instance and most referenda, the nonpartisanship. Evidently referenda alone, even on critical issues, will not move most people to the polls.

Consequently turnout is less a function of the issue than of the significance of the concurrent election. I have observed this regularity in fiscal referenda and its frequent bearing on outcomes.[55] From our knowledge of the class and racial bias in voting and the foregoing data on

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proposals and 25% of school operating levies were defeated. Memorandum of Ohio Education Association.

[51] Typical of mass rejection of civic elite aspirations was the simultaneous rejection by the Lansing electorate of a bond issue for a civic center and an ambitious scheme for incorporating the hinterland township, April, 1953. For an account of the repeated defeats at the polls of elite efforts to establish metropolitan government in Cleveland and St. Louis, see Watson and Romani, *op. cit.*, and Scott Greer, *Governing the Metropolis* (New York, 1962). Greer asserts that the fundamental obstacle to rationalizing the political structures of metropolitan areas is Jacksonian notions of self-determination, exemplified and enforced by the referendum.

[52] Edward D. Banfield and James Q. Wilson, *City Politics* (New York, 1963), p. 324; James M. Burns and Jack W. Peltason, *Government by the People* (Englewood Cliffs, 1966), p. 721.

[53] John C. Mueller, *op. cit.*

[54] Reo M. Christenson, "The Power of the Press—The Case of the Toledo Blade," *Midwest Journal of Political Science*, 3 (1959), 227–240. The Blade deviated from its characteristic enthusiasm for civil rights, because its editors perceived that the city elites could not win the referendum. Its aloofness, except for a lame last minute endorsement, was a great blow to the FHO proponents and a self-fulfilling prophecy.

[55] The approval rate of school tax levy and bond referenda in Ohio is higher in general elections than in primaries and specials. Thus a school levy is defeated in May and wins in November. Cf. Byron H. Marlowe, "An Explanation of Voter Behavior in Tax Elections" (paper, 1969 meeting, American Educational Research Association).

the differences between voters and nonvoters in the Toledo case, it follows that the accuracy of the referendum as an index of community opinion is maximized when the plebiscite is concurrent with a presidential election and that an isolated one or one concurrent with an insignificant election is likely to be a distorted reading and possibly erroneous if the vote is close and turnout is low.

Direct democracy, whatever its debits, presumably has the august virtue of assuring majority rule. The repeated defeats of open housing enactments in referenda are incontrovertible evidence that representative democracy produces occasional instances of authentic minority rule. One wonders how many instances would be exposed if the I & R were more widespread. However, direct government itself is governance by a minority, only 22% of the population in the Toledo referendum, and that active minority may be far from a mirror image of the populace; mathematically there is room for minority rule in a low vote plebiscite.

There is a defense for any incongruence of the legal electorate and the actual electorate in a referendum. One might say that it is an automatic adjustment for intensity of policy preferences, a solution of "the intensity problem."[56] The equality of the ballots ignores intensity and therefore might fail to optimize social utility, so fluctuating turnout provides the adjustment for intensity. Can it be said that turnout operated to maximize social utility in the Toledo referendum when Negro voting was below white because of pessimism, poverty, and status? In view of the class and racial bias of voting, can one expect low turnout to be a corrective for the intensity problem? In fact Wolfinger and Greenstein cite this as a principal defect of plebiscitary decision making. Legislators are responsive to intensity, but voters are less disposed or able to make such calculations and every voter's preference is weighted equally.[57]

The standard assertion that direct democracy is defective because its homo politicus premise is fallacious, i.e., that the populace lacks the presumed interest and requisite information, finds some support in this instance, a referendum in which the conditions for interest and acquisition of knowledge appear to be about optimal. The data suggest a dilemma for plebiscitary theory. If one points to the superior knowledge of the voters in the sample as evidence that the effective electorate is better informed, then one is snagged on the other horn. To the extent that plebiscitary voting is well informed, it violates its own universalist ethic and vice versa. Of course this is a general dilemma, but surely it is more critical for direct than representative democracy.

Unless one equates a raw majority of private preferences with the public interest, a serious shortcoming of direct legislation is the likelihood that many ballots are tallies of private interest rather than judgments, informed or uninformed, of the public interest. It would seem that when the electorate supercedes the legislature, it also is acting in a representative capacity, being only a minor fraction of the community, and has the concomitant responsibilities of the acquisition of knowledge, consideration of intensities, deliberation, and an earnest quest for the public interest. Those are formidable tasks which many of the Toledo electors did not undertake.

There will be opportunity to take more soundings, because plebiscitary democracy apparently is cherished in those jurisdictions that have it. Responses of the Toledo sample to queries regarding the institution displayed intense populism; the institution was favored by an eight to one margin and four-fifths viewed the FHO as an uncomplicated question and proper for decision by plebiscite rather than city council, although simultaneously most respondents acknowledged a deficiency of information for doing it.[58] Significantly, the pro-FHO respondents also were favorable to plebiscites. Apparently fifty years later and despite talk about the complexity of public policy, there was little doubting of the Progressive rationale of direct legislation as the instrument for an enlightened citizenry to correct errors of unrepresentative, myopic, or venal legislatures. For students of politics the open housing referenda offer intriguing questions. Is direct democracy a superior instrument for policy making? Who is enlightened or myopic? Would the Progressives be gratified by this use of their nostrum?

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[56] Cf. Willmoore Kendall and George W. Carey, "The 'Intensity' Problem and Democratic Theory," this Review, 62 (1968), 5–24.

[57] *Op. cit.*, 768.

[58] The items were: Attitudes Regarding Referenda

| | SA | A | D | SD | DK | NA |
|---|---|---|---|---|---|---|
| "The City Council should decide all local issues instead of having elections on issues." | 3 | 8 | 53 | 27 | 6 | 2 |
| "If all Council members have voted for an ordinance, the people ought to vote for it too." | 6 | 18 | 57 | 10 | 6 | 3 |
| "We should not have an election of the Fair Housing because that stirs up passions." | 3 | 18 | 55 | 11 | 10 | 3 |
| "The FHO is so complicated, it should be decided only by the city council, not by an election." | 3 | 9 | 55 | 24 | 7 | 3 |
| "I feel that I need more information in order to vote wisely on the Fair Housing questions." | 9 | 45 | 35 | 4 | 4 | 3 |

# A COMPARATIVE ANALYSIS OF SENATE HOUSE VOTING ON ECONOMIC AND WELFARE POLICY: 1953–1964*

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The manifest purpose of the roll call analysis described in this paper is that of demonstrating the existence of two policy dimensions in Congressional voting: economic and welfare. Support is sought for two propositions:

I. Each of the two dimensions appears in both the House and the Senate in each of six Congresses, the 83rd through the 88th, 1953–1964;
II. Roll call voting on the economic policy dimension is more heavily influenced by partisan differences while welfare policy voting is more subject to constituency constraints.

The second proposition is significant as an attempt to distinguish between a policy dimension on which partisan differences appear to be responsible for the greater part of the voting variation, and a policy dimension on which constituency factors have a substantial impact. This bears upon the *more general concern with distinguishing those party differences in voting behavior which are a function of an independent partisan factor from those which may be attributed to any number of factors correlated with partisan affiliation.*[1] This problem will be viewed from different analytic perspectives, including an analysis of the effects of intra-party and inter-party personnel turnover on the policy positions taken by representatives of the same constituency.

Instrumental to the testing of the propositions advanced above, but of independent interest as well, are two analytic themes: (1) the Senate-House comparison, and (2) the longitudinal analysis of legislative voting behavior. The Senate-House comparison is relatively unique in that such comparisons have seldom been undertaken; to our knowledge, none have been published using the type of measures employed in this study.[2] In the Senate-House comparison, the substantive interest will be foremost, although certainly more advanced comparisons of Senate-House voting will involve methodological issues of some complexity. In contrast, the longitudinal component of the present study involves a heavier methodological investment, including an evaluation of the evidence that the voting dimensions measured in successive Congresses are the same throughout. This evaluation will be conducted according to criteria specified in a model of measurement identity proposed earlier by one of the authors.[3] In addition, we shall consider the meaning and the utility of the types of voting measures to be constructed. The latter consideration arises out of a particular orientation to the use of roll call voting data.

## I. ORIENTATION

Legislative roll call analysis is commonly identified as the province of the specialist in the study of the legislative process. This can evoke rather tedious discussions about the merits of using roll call analysis to obtain an understanding of the manner in which legislative bodies process demand inputs.

It is our view that roll call analysis is equally, if not more, relevant to the study of public opinion and electoral behavior and other studies which focus upon the relationship between input variables that embrace the political system and output variables, as represented by

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[1] See Duncan MacRae, Jr., "Partisanship and Issues in Congressional Voting," Paper delivered at the *1968 Annual Meeting of the American Political Science Association,* September 2–7, Washington, D.C. In relation to the differentation of the impact of constituency and partisan factors see Lewis A. Froman, Jr., *Congressmen and Their Constituencies* (Chicago: Rand McNally and Company, 1963).

[2] Senate-House comparisons of roll call voting have been made utilizing other methods, e.g., David B. Truman, *The Congressional Party* (New York: John Wiley and Sons, Inc., 1959); Lewis A. Froman, Jr., *op. cit.,* pp. 69–84; H. Bradford Westerfield, *Foreign Policy and Party Politics* (New Haven: Yale University Press, 1955).

[3] Aage R. Clausen, "Measurement Identity in the Longitudinal Analysis of Legislative Voting," this REVIEW, 61 (December, 1967), 1020–1035.

roll call voting measures, which are also systemic in scope by virtue of the representational character of the members of a legislative body. This view of the utility of roll call voting analysis is particularly appropriate *if the research strategy is one of developing measures of voting which represent the broader themes of policy cleavage such as economic policy and social welfare.* Such cleavages present the maximum potential for ordering Congressmen *and* other actors in the political system.

Clearly, there are limitations to this analytic approach. An ordering of legislators on a policy dimension does not give any measure of the magnitude of the legislative output. Secondly, a portion of the legislative output is the product of transient combinations of forces that emerge at different points in the legislative process and is not subsumed under the broader policy dimensions. Thirdly, roll call voting measures, taken as indicators of the total set of activities through which legislators affect policy, are lacking somewhat in reliability and validity.

With respect to the third limitation, it is our view that the weakness of voting measures as indicators of total legislative activity increases with a narrowing of the policy domain. Where the purpose of the voting analysis is to order legislators on broad policy dimensions, encompassing a variety of specific issue questions, the resultant measure is less subject to measurement error. Here we are proposing that the *mean* tendencies for individual legislators' voting acts will correspond to the *mean* tendencies of other legislative activities having to do with the full set of legislative proposals encompassed by the measure of a broad policy dimension.

The strategy of focusing roll call studies upon broad policy dimensions facilitates historical, as well as comparative, research for it enables one to use the policy domain as the comparative base. Thus, in preparation for the statistical analysis of the relations among roll calls, preliminary conceptual definitions of policy domains are used to sort roll calls into broad policy categories. Within each of the categories, the dimensional analysis is then undertaken. In the present study, the first part of which is presented in this paper, five policy domains have been defined: foreign, farm, civil rights and liberties, welfare, and economic policy. Within each of these, an interpretation of the policy content of the emergent dimensions is possible without an undue strain on the imagination. With the issue content fairly clearly defined, the analysis can be extended horizontally, as well as longitudinally.

In contrast, when a correlational analysis, such as cluster or factor analysis, is applied to the entire set of roll calls in a particular session (or Congress), one is all too often faced with roll call groupings which are a substantive potpourri. Granted, an interpretation may be placed on these clusters in terms of the variety of factors which produced alignments on diverse issues that meet the terms of the statistical model used to establish dimensionality. However, comparisons over time, and across legislative bodies, may turn out to be exceedingly difficult because of the substantive complexity of the measures. In addition, there is the possibility that policy voting dimensions, which are conceptually distinct, may converge statistically at given points in time. This is to say that policy dimensions can not always be objectively identified in the statistical relationships between roll call votes.[4]

We wish to reassure our readers that while using preliminary definitions of policy domains, we rely upon objective statistical analyses of the roll call relationships *within* each policy domain to provide the basis for the construction of the measures of the policy dimensions. In addition, we consider the correlations *between* measures constructed in the various domains, in search of policy dimensions extending across policy domains, and between the voting measures and relevant variables. This provides a substantial body of evidence on which to base final interpretations.

## II. MEASUREMENT AND IDENTIFICATION OF POLICY DIMENSIONS

As mentioned above, the analysis reported here is confined to two domains: welfare and economic policy. The basis for the distinction between the two domains is provided by an earlier analysis of House voting in the 83rd through the 86th Congress, which was based on a much smaller subset of votes than that in the present study.[5] The content of the domains will be indicated later.

### *Clustering and Scoring Formats*

Within each of the policy domains, a cluster analysis of the roll calls was performed for both houses in each Congress (across parties). This meant twenty-four independent cluster analyses on roll call subsets ranging in size from 24 to 138 roll calls. Clusters were defined by the criteria that the average Yule's Q correlation among all roll call pairings should be .70 or greater, and that no individual correlation should be less than .60.[6]

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[4] Duncan MacRae, Jr. and Susan Borker Schwartz, "Identifying Congressional Issues by Multidimensional Models," in *Midwest Journal of Political Science*, 12 (May, 1968), 181–201.

[5] Clausen, *op. cit.*

The clustering format represents a compromise between two goals: (1) producing clusters, and subsequently measures, which are relatively homogeneous, and (2) producing roll call voting measures which may be statistically differentiated in a subsequent correlation analysis. If the correlation criteria are set too high, a number of clusters may be identified which are so highly correlated that the measures constructed on them can not be differentiated in their relationships to other variables. If the correlation criteria are set too low, homogeneity is reduced and interpretation becomes more difficult.

The cluster analysis provided a check on our definition of policy domains by requiring that objective statistical criteria are met before measures of policy dimensions are constructed. Once the roll call clusters had been defined, the next step was that of constructing the voting measures on which the legislators would be ordered. We used a scoring procedure in which a positive vote was scored 1, a negative vote scored 3, and the absence of a recorded vote or announced position scored a 2. The positive vote, or position, was arbitrarily designated for one roll call in the cluster, with votes on all other roll calls in the same cluster assigned positive and negative positions so as to produce positive correlations among all roll calls. A mean score was computed for each legislator whose position was known on 50 percent or more of the roll calls in a given cluster.[7] The individual scores vary from 1 to 3 and are indicators of the relative proportion of positive and negative responses, with a regression toward the score of 2 as a function of absences.[8]

### *Cluster Analysis Results*

The cluster analysis of the welfare and economic policy roll calls in the House and Senate produced a dominant cluster within each domain, in each house, and in each Congress. This "primary" cluster always contained over twice the number of items in the next largest cluster and, on the average, accounted for 65 percent of the roll calls in the domain.

These results provided an early indication of a major policy dimension within each domain that extended across the six Congresses. This was also indicated by the continuity of the legislative content of the primary clusters over time. Furthermore, a comparison of the content of the primary clusters in the two bodies shows a high degree of similarity between the House and the Senate.

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[6] The .70 correlation criterion is approximately equivalent to the standard .90 level of reproducibility used in Guttman scaling. However, it should be noted that the requirement of a minimum of .60 for individual roll call pairings has, in practice, yielded clusters dominated by inter-item correlations of .80 and above. The setting of a minimum correlation of .60 for individual correlations guards against the possibility of an item with an extreme division being included in a cluster despite wildly fluctuating correlations that average out to .70 or better. The exclusion of such items is justified on the grounds that highly variable correlations are poor indications of a common underlying dimension. Actually, this danger had already been minimized by excluding items more extreme in their division than 90–10. For a discussion of the use of Yule's Q in roll call analysis, see Duncan MacRae, Jr., "A Method for Identifying Issues and Factions from Legislative Votes," this REVIEW, 62 (March, 1968), 909–926.

[7] The scoring was performed using a program constructed by Keith Billingsley.

[8] There are two aspects of this scoring procedure which require explanation in terms of the assumptions made: (1) the scoring of absences, and (2) the simple summation scoring. One alternative to our scoring of absences would be that of excluding absences in the scoring. This assumes that the roll calls on which no absences are observed, with respect to each legislator, are a representative sample of the set of roll calls in the scale. We prefer not to make this assumption, treating the absence instead as partially a function of indifference or abstention. In each case, a middle scoring for the legislator is a reasonable option although more elaborate procedures might be used such as scoring absences according to a mean computed for the entire body on the given roll call.

The second feature of the scoring procedure, the simple summation of values across roll calls, recognizes that individuals occupying the same position on a policy dimension may have somewhat different voting patterns. They are assumed to be alike only in the probabilities of voting Yea and Nay, probabilities which vary between 0 and 1 probabilities assumed by the Guttman scale model. Only across a number of roll calls will the order positions on the dimension become clear as individual idiosyncracies are absorbed into the more general pattern of voting.

Actually, the scoring technique does not produce results which deviate sharply from those obtained by the Guttman scoring method. The scoring procedure does relax the deterministic response criteria of the Guttman model. On the other hand, the requirement of high Yule Q correlations among roll calls places constraints on the variations in the voting patterns of individuals assigned the same score. For a discussion of probability models and the simple summation scoring of monotone items, see Warren S. Torgerson, *Theory and Methods of Scaling* (New York: John Wiley and Sons Inc., 1958), pp. 360–402.

In the Senate-House comparison, a distinction is made between the content of the roll calls identified with the primary clusters in each domain, and the set of votes associated with all other clusters. The comparison is based on those content categories in which ten or more roll calls were taken some time during the six Congresses in at least one house; in addition, as an aid to interpretation, we have also included all those categories exclusively identified with either the primary or the "other" clusters, in either house (Table 1).

A distinct pattern emerges in the association of content categories with the primary and other clusters in the two houses. This pattern shows the content similarity of the two sets of clusters across the two houses and reflects differences in the two institutions. The pattern is Guttman-scalar in form: (1) there are categories for which the primary cluster is dominant in both houses, (2) categories for which the primary cluster is dominant in the House but not in the Senate, and (3) categories where the primary cluster is dominant in neither house. In only one category, old age pensions and public assistance, is the primary cluster dominant in the Senate but not in the House. Thus, the difference in the cluster structures in the two bodies is that the Senate produces more roll calls in the content categories which are not assigned to the primary cluster.

The institutional differences reflect the fact that the Senate takes more roll calls than the House and entertains a greater variety of amendments. The more detailed consideration of legislation in the Senate increases the possibility of a greater differentiation in the substantive issues which are brought to a vote and correspondingly, a greater complexity in the dimensional structure.

The evidence on the content similarity of the primary clusters within each domain encourages us to propose that there is a policy dimension in each policy domain which is the same in both houses. Central to the hypothesized economic policy dimension are the recurring topics of public power, atomic energy, deficit financing, housing mortgage and interest policy, and public works (including anti-recession public works spending). Topics less frequently voted on but exclusively identified with the economic policy dimension are air and water pollution, anti-trust, sale of government-owned rubber plants, wage-price controls, 160 acre limit on federal irrigation allotment per user, and the transfer of government civilian work to private industry. The issues involved here are most familiar as ones related to the conflict between *liberal, public-oriented economic policy* and *conservative, business-oriented economic policy:* government ownership and regulation of economic enterprise, government spending versus private incentives to economic stability and growth, tight versus easy credit policy, and the private versus the public development of natural resources.

The welfare policy dimension is dominated by legislation on public housing and urban renewal, labor regulation, education, and employment opportunities and rewards. The tight identification of legislation on tidelands oil rights and federal pre-emption of state legislative powers with the welfare dimension may appear somewhat strange. However, there are indications that welfare policy considerations are involved in both cases. Thus, with respect to the pre-emption issue, some Congressmen argued that those who supported states' rights had also been those opposed to federal social welfare legislation.[9] With regard to the tidelands oil controversy, the desire for federal control of oil revenues for social welfare purposes is evident in proposals to use the revenue for federal aid to education.[10]

In general, legislation on the welfare dimension involves a relatively direct intercession of the government on behalf of the individual, cushioning him against the jolts administered by the economy, assisting him in coping with more powerful economic elements, and aiding him in getting the equal chance that the ideal of equal opportunity demands. Economic policy is relatively less direct in its impact upon the individual; it is more concerned with the provisions to be made by the government for maintaining the economy on an even keel, supporting its development in particular areas, and defining the public interest in relation to private economic interests.

The proposition that two dimensions, economic and welfare policy, are being measured by the primary clusters in the two policy domains requires further testing. The criteria to be used are drawn from a *model of measurement identity* proposed by Clausen (see fn. 3). One of the three criteria proposed, that of content similarity in the roll calls included in measures of the same dimension, and differentiation of content across dimensions, has already been considered. The other two criteria involve: (1) the

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[9] *Congress and the Nation: 1945–1964* (Washington, D.C.: Congressional Quarterly Service, 1965), pp. 1398–1399.

[10] Ibid., pp. 1205, 1403–1404.

TABLE 1. ROLL CALL CLUSTER ASSIGNMENTS (83RD–88TH CONGRESS)

| Content Categories | House | | Senate | |
|---|---|---|---|---|
| | Primary | Other | Primary | Other |
| **A: Economic Policy** | | | | |
| Water & Air Pollution | 10 | — | 4 | — |
| Sale: Govt. Rubber Plts. | 4 | — | 6 | — |
| Anti-trust, Monopoly | 4 | — | 1 | — |
| Govt. Civ. Work to Pvt. Industry | 4 | — | 1 | — |
| 160-acre Limit: Fed. Irrig. Allot. | 1 | — | 1 | — |
| Wage-Price Controls | — | — | 6 | — |
| Public Power | 14 | — | 16 | 1 |
| Atomic Energy | 14 | 3 | 22 | 0 |
| Deficit Financing | 16 | 3 | 13 | 3 |
| Hous. Mortgage & Interest | 4 | — | 18 | 2 |
| Pub. Wks. & Anti-Recession Programs | 26 | 2 | 34 | 10 |
| Individual Income Taxes | 2 | — | 10 | 7 |
| Corporation Taxes | 1 | — | 8 | 5 |
| Investment Taxes: Indiv. & Corp. | 2 | — | 10 | 17 |
| Excise Taxes | 2 | — | 7 | 17 |
| Maritime: Subsidy & Regulation | 5 | 1 | 7 | 20 |
| Conservation, Parks, Forests | 6 | — | 2 | 8 |
| Lake Mich. Divers-Ill. Wtwy. | 6 | 1 | 1 | 10 |
| Reclamation Projects | 13 | 8 | 3 | 5 |
| Commun. Satellite Corp. | — | — | 21 | 15 |
| Oil-Gas Industry: Tax & Regulation | — | 3 | 1 | 13 |
| St. Lawrence Seaway | — | — | — | 3 |
| Anti-trust: Agricultural Coop. | — | — | — | 4 |
| Beautification Program | — | — | — | 2 |
| Decentralization: U.S. Industry | — | — | — | 2 |
| **B: Welfare Policy** | | | | |
| Tideland Oil Rights | 4 | — | 13 | — |
| Dept. of Urban Affairs | 1 | — | 1 | — |
| Federal Prempt. State Legis. | 7 | — | — | — |
| Pub. Hous., Urban Renewal | 19 | 1 | 29 | 1 |
| Fed. Pay & Pension: Lower Civ. Grades | 17 | — | 17 | 3 |
| Labor Regulation | 12 | — | 44 | 7 |
| Unemployment Compensation | 5 | — | 15 | 1 |
| Economic Opportunity Act | 5 | 1 | 9 | 1 |
| Minimum Wage | 6 | 2 | 19 | 1 |
| Education (incl. Voc. Rehab.) | 26 | 4 | 50 | 15 |
| HEW & Labor Dept. Funds | 10 | — | 5 | 3 |
| Migrant Labor Aid & Regulation | 4 | 1 | 7 | 10 |
| Medical Serv., Trng. & Research | 6 | 4 | 26 | 10 |
| Public Safety | — | — | — | 2 |
| Education: Planning & Research | — | 3 | — | — |
| Humane Slaughter | — | — | — | 2 |
| Old Age Pensions & Pub. Assistance | 2 | 2 | 11 | 2 |

correlations among the scales measuring the two dimensions, and (2) the correlation of those scales with other variables. We shall apply the first of these two criteria in the next section.

This criterion states that *measures of the same dimension at different points in time should be more highly correlated than measures of different dimensions.*

*Inter-scale Correlations*

Inspection of the inner-scale correlation matrix shows the degree to which the present data fit the measurement identity criterion for inter-scale correlations (Table 2).[11] In the House this criterion is reasonably well-satisfied (remember that small differences in correlations are much more significant among upper range values such as these than they are for lower range values—simply think in terms of $r^2$ rather than r). The one scale in the House which clearly fails to meet the inter-scale correlation requirement of the measurement identity model is the 84th welfare scale. This scale has consistently higher correlations with economic scales than with welfare scales.

In the Senate, the 83rd welfare, and the 87th and 88th economic scales fall short of expectations. The 88th economic policy scale is most deviant; its correlations with welfare scales are, with one exception, higher than its correlations with economic policy scales.

When the measurement identity criterion for inter-scale correlations was proposed it was recognized that there was one particular difficulty with this criterion. It assumes that legislators will not shift their positions appreciably from one Congress to the next. If such movement does occur, the correlations between measures of the same dimension will be reduced. Concommitantly, correlations between measures of different dimensions may either increase or decrease. This weakness of the criterion becomes most apparent when a distinction is sought in the measurement of two dimensions as highly correlated as in the present case. We shall return to this problem later when we consider the regional-party alignments on the two dimensions. Let us turn now to the remaining test of measurement identity, the correlation of the scales with other variables.

*Voting Correlate Analysis*

The measurement identity model states that *measures of different dimensions will be different in their correlations with other variables, and that the pattern of correlations will remain constant over a period of time although the magnitudes of the correlations may vary.* Thus the case may be that measures of dimension A are more highly correlated with variable X than measures of dimension B, whereas measures of dimension B are consistently more highly correalted with variable Y. Yet the absolute magnitude of the correlations may vary. Take, for example, the case in which the party lines are generally more sharply drawn in a particular Congress. This would produce higher correlations of party across *all* party related dimensions, yet the *relative* magnitudes of the correlations of party with different dimensions would be expected to remain constant. Let us turn now from the hypothetical illustration to consider, first, the correlation of party with the measures of the two dimensions and, second, the relationships of constituency characteristics to the two dimensions.

The relationship of party to welfare and economic policy voting is presented in terms of the proportion of scale variance "explained" by party (Table 3). An entirely consistent pattern emerges with the proportion of scale variance explained by party being higher in each Congress for the economic policy scales.

The distinction between the two dimensions is particularly sharp in the House, with the difference in the "explained" scale variance ranging from .15 to .62, producing a mean difference of .38. In the Senate, though the explained variance differences are less sharp, they are consistent, varying from .07 to .34 with a mean difference of .21.

The consistent difference in the magnitude of the correlations of party with the economic and welfare policy scales support the proposition that two distinct dimensions are being measured by the scales in the two houses across six Congresses. The maintenance of the difference in the party associations, in the face of deviations from the expected inter-scale correlation pattern, suggests a repositioning of members within either one or both of the parties, while the overall relationship between Democrats and Republicans remains relatively constant. Further support for the welfare-economic policy distinction is found in the relationship of constituency characteristics to voting positions.

Two constituency characteristics, urbanization[12] and blue collar percentage of the labor force,[13] were correlated with economic and

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[11] The correlations reported here and in the remainder of the paper are Pearson r's.

[12] Urbanization measure on Congressional districts is percentage urban minus percentage rural, based on data provided by "Congressional Quarterly Weekly Report, No. 5" (February 2, 1962), pp. 156–159. Urban areas contain a central city of 50,000 or larger, including suburban areas containing a city of 100,000 or larger; suburban areas consist of closely settled areas contiguous to central cities; rural areas contain cities smaller than 50,000. The urbanization index for states is a weighted composite of the district indices.

[13] Percentage blue collar measure on Congressional districts is based on data provided by "Congressional Quarterly Weekly Report, No. 29" (July 20, 1956), p. 853. The blue collar definition in-

TABLE 2. ECONOMIC AND WELFARE SCALES: INTER-CORRELATIONS

| | | House | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | Welfare | | | | | Economic | | | | | |
| | | 84 | 85 | 86 | 87 | 88 | 83 | 84 | 85 | 86 | 87 | 88 |
| WELFARE | 83 | .81 | .88 | .90 | .87 | .82 | .81 | .81 | .74 | .83 | .81 | .88 |
| | 84 | | .74 | .74 | .73 | .67 | .85 | .88 | .87 | .86 | .85 | .81 |
| | 85 | | | .94 | .92 | .87 | .65 | .70 | .64 | .79 | .78 | .81 |
| | 86 | | | | .95 | .90 | .67 | .73 | .67 | .84 | .83 | .87 |
| | 87 | $\bar{x} = .84$ | | | | .93 | .66 | .72 | .67 | .81 | .84 | .88 |
| | 88 | | | | | — | .59 | .65 | .62 | .76 | .81 | .89 |
| ECONOMIC | | | | | $\bar{x} = .77$ | | — | .95 | .93 | .88 | .90 | .82 |
| | 84 | | | | | | | | .96 | .93 | .93 | .86 |
| | 85 | | | | | | | | | .92 | .93 | .83 |
| | 86 | | | | | | | $\bar{x} = .91$ | | | .96 | .92 |
| | 87 | | | | | | | | | | | .96 |
| | 88 | | | | | | | | | | | — |

| | | Senate | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | Welfare | | | | | Economic | | | | | |
| | | 84 | 85 | 86 | 87 | 88 | 83 | 84 | 85 | 86 | 87 | 88 |
| WELFARE | 83 | .83 | .85 | .89 | .90 | .80 | .87 | .93 | .93 | .92 | .86 | .85 |
| | 84 | | .93 | .91 | .91 | .85 | .80 | .88 | .89 | .87 | .84 | .95 |
| | 85 | | | .91 | .90 | .83 | .82 | .89 | .90 | .89 | .84 | .91 |
| | 86 | | $\bar{x} = .89$ | | .98 | .95 | .83 | .87 | .86 | .85 | .87 | .92 |
| | 87 | | | | | .94 | .83 | .86 | .84 | .83 | .87 | .94 |
| | 88 | | | | | — | .67 | .73 | .71 | .71 | .82 | .91 |
| ECONOMIC | 83 | | | | | $\bar{x} = .85$ | — | .96 | .94 | .95 | .85 | .80 |
| | 84 | | | | | | | | .97 | .97 | .85 | .87 |
| | 85 | | | | | | | | | .96 | .86 | .84 |
| | 86 | | | | | | | $\bar{x} = .89$ | | | .88 | .82 |
| | 87 | | | | | | | | | | | .83 |
| | 88 | | | | | | | | | | | — |

TABLE 3. VARIANCE ON ECONOMIC AND WELFARE SCALES EXPLAINED BY PARTY

| Congress | Senate | | House | | Difference Economic-Welfare | |
|---|---|---|---|---|---|---|
| | Welfare | Economic | Welfare | Economic | Senate | House |
| 83rd | .59 | .72 | .48 | .83 | .13 | .35 |
| 84th | .53 | .83 | .71 | .86 | .30 | .15 |
| 85th | .56 | .74 | .28 | .90 | .18 | .62 |
| 86th | .42 | .76 | .37 | .76 | .34 | .39 |
| 87th | .46 | .53 | .37 | .79 | .07 | .42 |
| 88th | .32 | .52 | .37 | .71 | .21 | .34 |
| $\bar{x}$ | *.48* | *.68* | *.43* | *.81* | *.21* | *.38* |

welfare policy measures for each house and within each party (Table 4). The pattern conforms to the measurement identity model criterion, with the three exceptions noted in the table. It is a pattern in which measures of the welfare policy dimension are more positively correlated with the constituency variables than are measures of the economic policy dimension. This observation encompasses those instances in which "more positive" means "less negative."

An interesting aspect of these data is the strength of the *negative* relationship between urbanization-blue collar and liberal economic policy during the Eisenhower administration among Republican Senators. This relationship also holds among House Republicans, but at a much lower level.

The negative relationship between urbanization-blue collar and liberal economic policy among Republicans during the Eisenhower administration is reminiscent of the urban-rural, East-West, pro-business-anti-business political theme extending back at least as far as the populism of the post-Civil War period of the 19th century. In this view, Republicans from urban areas respond to the dominant financial and business interests in their constituencies while those from more rural areas oppose an economic policy designed to benefit those interests. The strength of this relationship during the Eisenhower administration is understandable to the extent that pro-business sentiments so clearly expressed by Eisenhower served to accentuate this historical cleavage.

Whatever the validity of these interpretations, the evidence is circumstantial. We are more impressed with the implications of the full set of data on both parties for the proposition that *increasing urbanization-industrialization is generally productive of forces that demand a liberal public policy, or what is thought of as liberal policy.*[14] This proposition appears tenable for social welfare when the relevant constituency variables are assumed to be characteristics of the occupational and ecological structures of the constituencies. However, when the policy at issue is economic, the relevant constituency characteristics may be, to a larger extent, the financial and corporate structure of the constituency. The heavier concentration of financial and industrial organizations in the urbanized areas implies a conservative attitude on economic policy, independent of the attitude on welfare policy, which may be much more liberal. Although the influence of these conservative constituency elements would appear to be most effective with respect to Republican legislators, the lower correlation of urbanization-industrialization with economic policy, relative to welfare policy, observed among Democrats is entirely consistent with this viewpoint.

Another perspective on the voting alignments on economic and welfare policy dimensions is provided by a regional-party breakdown. In this breakdown, Democrats and Republicans were sorted into three groups: *Coastal* (Northeast, Middle Atlantic, Far West), *Interior* (North Central, Border, Mountain), and *South*.[15] For each regional-party grouping a weighted mean score was computed for the four Congresses of the Eisenhower administration (83–86) and the two Congresses serving under Kennedy and Johnson (87–88).[16] The results are shown in Figure 1, where the mean scores are represented by the center-most extensions of the bars in a

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cludes craftsmen, foremen, machine operators, private household help, service employees, and all laborers except those who work on farms. The state measure of blue collar is a weighted composite of the district indices.

[14] This proposition is implied at different points in the literature but we have found no author who states it clearly enough to warrant citation.

[15] *Coastal:* Alaska, Hawaii, Connecticut, Maine, Massachusetts, New Hampshire, Rhode Island, Vermont, Delaware, New Jersey, New York, Pennsylvania, California, Oregon, Washington; *Interior:* Illinois, Indiana, Michigan, Ohio, Wisconsin, Iowa, Kansas, Minnesota, Missouri, Nebraska, North Dakota, South Dakota, Kentucky, Maryland, Oklahoma, Tennessee, West Virginia, Arizona, Colorado, Idaho, Montana, Nevada, New Mexico, Utah, Wyoming; *South:* Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Texas, Virginia.

[16] Mean scores computed on the individual Congresses in each set are sufficiently homogeneous to warrant the aggregation for each set.

TABLE 4. CONSTITUENCY AND VOTING

| | Urbanization | | | | | | Blue Collar | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | All | | Rep | | Dem | | All | | Rep | | Dem | |
| | Welf | Econ | Welf | Econ | Welf | Econ | Welf | Econ | Welf | Econ | Welf | Econ |
| **House[a]** | | | | | | | | | | | | |
| 83rd | .33 | .12 | .19 | −.14 | .48 | .23 | .28 | .10 | .05 | −.28 | .44 | .16 |
| 84th | .20 | .16 | .13 | −.12 | .31 | .35 [b] | .20 | .10 | .19 | −.11 | .24 | .11 |
| 85th | .33 | .07 | .16 | −.12 | .47 | .17 | .25 | .02 | .23 | −.03 | .30 | −.07 |
| 86th | .36 | .15 | .22 | −.07 | .48 | .35 | .30 | .14 | .21 | .18 | .38 | .20 |
| 87th | .40 | .24 | .15 | −.07 | .51 | .42 | .31 | .16 | .21 | .11 | .36 | .20 |
| **Senate** | | | | | | | | | | | | |
| 83rd | −.08 | −.15 | −.39 | −.48 | .18 | −.15 | −.08 | −.17 | −.48 | −.64 | .08 | −.09 |
| 84th | .02 | −.11 | .02 | −.38 | .18 | .06 | .10 | −.05 | .15 | −.40 | .09 | .05 |
| 85th | .03 | −.09 | .00 | −.26 | .19 | −.02 | .00 | −.10 | .04 | −.23 | −.04 | −.11 |
| 86th | .16 | .01 | .06 | −.41 | .27 | .16 | .22 | .03 | .12 | −.61 | .19 | −.02 |
| 87th | .16 | .09 | .25 | .03 | .23 | .26 | .21 | .11 | .38 | .20 | .11 | .08 |
| 88th | .21 | .14 | .31 | .26 | .25 | .17 | .29 | .26 | .48 | .49 | .15 | .06 |

[a] Data on constituency characteristics for the 88th Congress comparable to that used on preceding Congresses was not available for the large number of new districts formed in the 88th.

[b] Denotes instances where urbanization-blue collar measures are more highly correlated with economic policy than with the welfare policy scale for that Congress.

format which shows the conservatism of each group. Both Senate and House voting patterns are shown, with Southern Republicans excluded due to insufficient cases. We can report, however, that Southern Republicans are at least as conservative as Coastal and Interior Republicans on all measures.

There are three sets of observations to be made: (1) the high degree of similarity between the Senate and House voting alignments within each of the Congressional periods, (2) the differences with respect to economic and policy voting alignments, and (3) the differences between the two Congressional periods. It will not always be possible to delineate one set of observations from the other in the presentation.

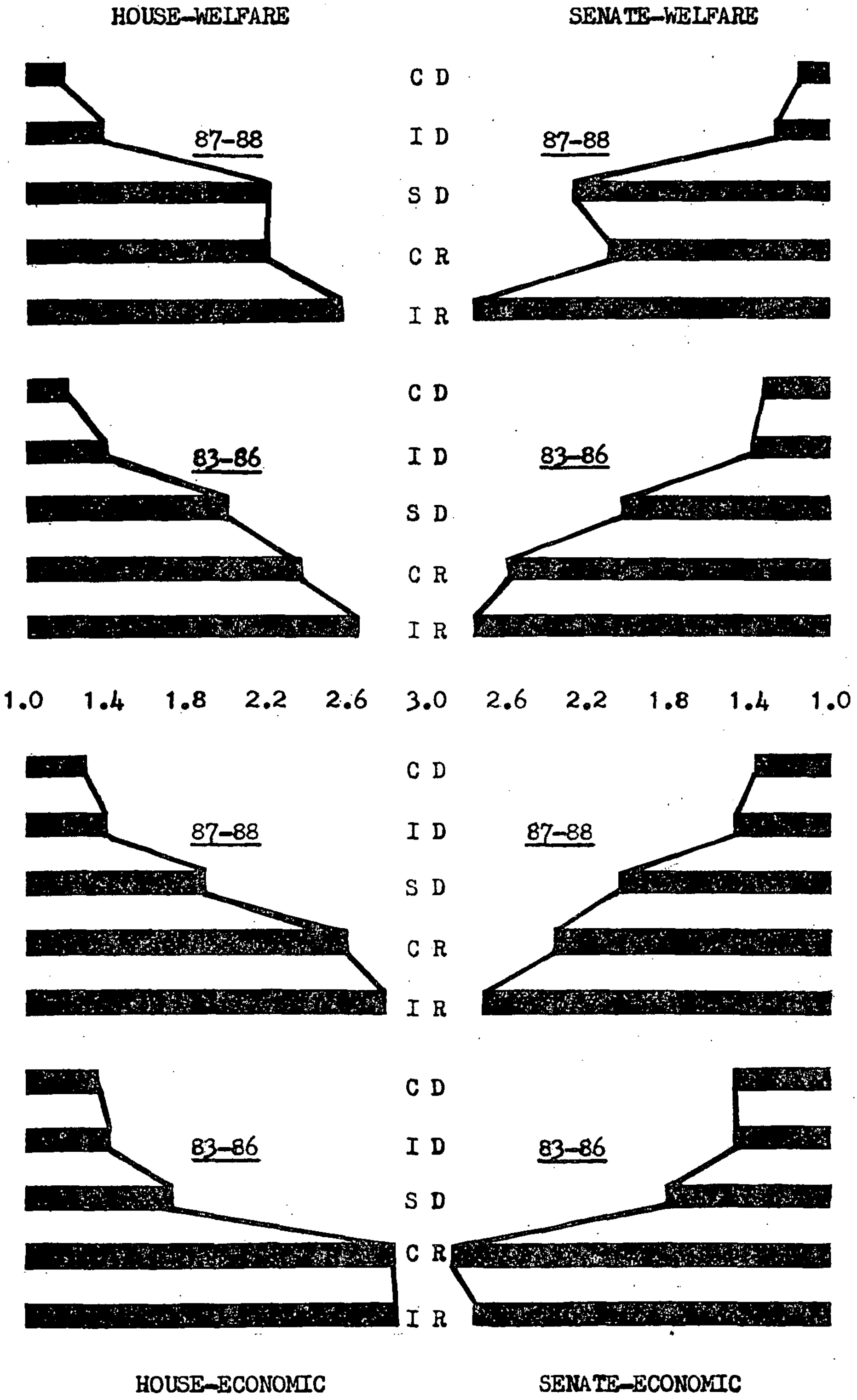
The similarity between Senate and House voting patterns in the two periods is most striking. It is all the more impressive because the similarity embraces both the differences between the economic and welfare policy alignments and the differences on each dimension observed between the Eisenhower and Kennedy-Johnson Congresses.

The parallel changes in the regional-party voting alignments, from the first to the second set of Congresses, on each dimension in both houses suggests that *the same set of forces are operative on Senators and Representatives*. This is suggested as well in the preceding analyses of the content of the roll calls associated with the two dimensions in the two Houses and the constituency and party characteristics of the orderings on the two dimensions.

Much of the change in voting patterns between the Republican and Democratic administrations is accounted for by the change in the relationship between the scores of the Coastal Republicans and other members of Congress. On economic policy under the Eisenhower administration, Coastal Republicans are as conservative as Interior Republicans in both Houses, whereas under a Democratic administration, they take a more liberal position. At a lower order of magnitude, Southern Democrats appear to be relatively more conservative during a Democratic administration.

The changes in the mean scores of Southern Democrats and Coastal Republicans is of the same form with respect to welfare policy in both the House and the Senate. However, the changes are more pronounced in the Senate where

FIGURE 1. Regional-party voting



Coastal Republicans actually become more liberal than the Southern Democrats in the 87th and 88th Congresses.

In general, the level of party unity among Coastal Republicans and Southern Democrats is higher on economic than on welfare policy. This difference between the two dimensions is particularly marked in the 83rd through the 86th Congresses.

It is appropriate at this time to return to a consideration of the deviations from the measurement identity model that appeared in the interscale correlations. The greatest deviation occurred with respect to the 87th and 88th Senate economic policy scales which were correlated as high, or more highly with welfare policy scales as with economic policy scales. This deviation now becomes explicable as a result of the behavior of Coastal Republican and Southern Democratic Senators. The change in the positions of these two sets of Senators, from the Republican to the Democratic administrations, produces a convergence between the 87th and 88th economic policy scales and the welfare scales.

However, this level of explanation for the statistical convergence of the economic and welfare scales in the Senate does not suffice to resolve the primary question: Is there a new economic policy dimension that appears in the 87th and 88th Senates, or is there a single dimension in these two Senates which includes welfare and economic policy roll calls, *or* do we stick to the proposition of bi-dimensional continuity throughout the entire period? We prefer the third alternative for the following reasons:

(1) The consistency of the other indicators (tests) in support of the original proposition.

(2) The fact that most of the change that occurred was on the part of a single regional-party grouping, Coastal Republicans, with some change on the part of Southern Democrats, while the other three groups remained fairly constant. If a new dimension had emerged one would have expected more diffusion in the re-ordering of positions.

(3) The turnover in the occupancy of the White House. This is considered to be more likely to produce changes in the positions taken by selected members of Congress than to alter the basic cognitive and attitudinal structures of the members. Such changes in position have been noted on foreign policy for the Truman-Eisenhower and Eisenhower-Kennedy turnovers.[17]

While on this issue we would like to comment briefly on the other two deviant scales: 83rd Senate and 84th House welfare. In the first instance, it is again the Coastal Republicans who are primarily responsible, for in this first Congress of the Eisenhower administration, and *only* in this Congress, they took a slightly more conservative position than their land-locked brethren. This then made that particular welfare scale resemble the economic scales in its voting alignment. Perhaps this was an expression of loyalty to the new Republican in the White House. Indeed, the whole pattern of Senate Coastal Republican voting reflects a certain level of sensitivity to the President. In the second instance, the 84th House welfare scale, it is the Southern Democrats who have a brief fling at strong party loyalty on welfare policy, with no discernible prior warning of this turn of events, and certainly no indication of any aftereffects.

The evidence in the present case leads us to conclude that there is a welfare policy and an economic policy dimension which extend across the six Congresses and are the same in both houses. This inference is based on the consistently higher correlation of party with the economic policy measures, the higher positive correlations of constituency characteristics with welfare policy scales, the different patterns of regional-party alignments on the two dimensions, and the pattern of inter-scale correlations. Comparability between the House and Senate is further supported by the content of the legislation associated with the two dimensions in the two bodies. The inference of measurement identity and differentiation is instrumental to the following analysis of stability and change with respect to individual constituency positions on the two dimensions.

### *Stability and Change*

An analysis of stability and change in legislative voting behavior has obvious implications for an understanding of the response of a representative legislative system to policy demands. What does past experience indicate to be the means through which change comes about: Is change in the representation provided a constituency effected by partisan turnover, by a change in the personnel without partisan turnover, or can it be observed for individual members serving in successive Congresses? What is the relative importance of these conditions for change from one policy dimension to the next?[18]

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[17] Mark Kesselman, "Presidential Leadership in Congress on Foreign Policy: A Replication of a Hypothesis," *Midwest Journal of Political Science,* 9 (November, 1965), 401–406.

[18] Previous studies concerned with the impact of personnel turnover include Lewis A. Froman, Jr., *op. cit.,* especially Chapter 8; and Lee Anderson,

There are three conditions to which our analysis of stability and change will direct its attention: (1) the same person serving in successive Congresses, (2) *intra*-party personnel turnover, and (3) *inter*-party personnel turnover.

Before the results of this analysis can be presented we must return once more to problems of measurement. The problem that confronts us is that of achieving scale scores which can be compared over time. It will be recalled that the original set of scale scores consisted of mean scores computed for each individual after a positive response, a negative response, and an absence on each roll call had been scored 1, 3, and 2 respectively. The comparison of such scores between time-1 and time-2 increases in validity as the roll calls included in the respective measures approach a representative sample of the universe of items relevant to a particular dimension. However, the set of roll calls in a given measure may be biased in one direction. Consequently, members whose positions have not changed may appear to have changed if one looks at their scale scores in successive Congresses. For example, if the set of items in Congress A are biased in the liberal direction compared to those in Congress B, members of Congress A are expected to have less liberal scores than members of Congress B, even though their true positions have remained unchanged.

In an attempt to control on this form of bias, the scores of all members in each Congress were adjusted to the mean score of the members who served continuously throughout the twelve-year period. The assumption is that changes in the collective mean score for the continuous members represent the effects of item bias in the measures used in the different Congresses. This involves the further assumption that the continuous members, *as a body*, have not shifted their position.

TABLE 5. MEAN CHANGE SCORES

| | Senate | | House | |
|---|---|---|---|---|
| | Economic | Welfare | Economic | Welfare |
| Continuous member (1) | .21 | .21 | .18 | .27 |
| Intra-party turnover (2) | .31 | .39 | .27 | .40 |
| Inter-party turnover (3) | 1.23 | 1.26 | 1.44 | 1.08 |

It is granted out of hand that this adjustment of scores falls short of an adequate solution to the problem of creating interval level scales that are invariant across Congresses. All we can say is that it seems more probable that there is sampling error in the item selection than that, in this particular case, 178 members of the House and 41 members of the Senate have moved *collectively* in a conservative or liberal direction. Indeed, the continuous members are as unlikely as any, because of their safe seats, to shift their positions in response to external pressures.

After the raw scores had been adjusted, change scores were computed for each pair of adjacent Congresses on each seat, and a mean change score computed across the five adjacent Congress pairings for each of three conditions: continuous member, intra-party turnover, and inter-party turn-over. The results are presented in Table 5.

The order of change is generally in line with expectations, individuals changing the least and inter-party turnover generating the most change. The change for individual members is little more than a tenth of the total range of scores on the individual scales. The low order of change observed for the intra-party turnover is understandable in that the same party and constituency constraints are operative on the new member as they were on the incumbent. The relationship between the two conditions, continuous member and intra-party turnover, can be represented by a ratio of the change observed for the latter condition to that for the former. This ratio allows for variation in measurement error across dimensions. In terms of this ratio, the level of intra-party change appears very similar across both dimensions and both houses. In three instances the ratio is 1.5; it inches up to 1.9 on the Senate welfare dimension.

Whereas the change associated with intra-party turnover is relatively constant across the two dimensions in the two houses, the inter-party turnover change scores vary substantially,

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"Individuality in Congress: A Research Note," *Midwest Journal of Political Science,* 8 (November, 1964), 425–429. Using a measure based on votes on reciprocal trade legislation, Froman concludes that districts represented by the same individual through several Congresses exhibit more stability than do districts characterized by a substantial turnover in personnel. He does not address the question of what occurs when the partisanship of the districts' representation changes. Anderson tests and confirms Froman's hypothesis, using both foreign involvement and domestic liberalism-conservatism scales. Also, see Clarence N. Stone, "Issue Cleavage Between Democrats and Republicans in the U.S. House of Representatives," *Journal of Public Law,* 14 (1965), 343–358, for a discussion of the impact of change in the partisan representation of a district. Stone suggests that the impact varies substantially among different policy areas.

and they are also of much greater magnitude.[19] Once again, let us consider these in terms of a ratio, inter-party turnover change to continuous member change. The largest ratio value (8.0) is observed on House economic policy, the smallest value (4.0) on House welfare policy, with both the economic and welfare policy ratios in the Senate, 5.8 and 6.0 respectively, falling midway between the two House ratios.

The particular value of longitudinal analyses is that they permit an approximation to experimental design in some instances. In the present case, we are emboldened to say something about the relative influence of partisan and constituency factors on welfare and economic policy voting. The evidence is provided by the change ratios associated with partisan turnover. The analytic design is as follows.

The unit of analysis is the constituency, with the member of Congress viewed as the medium through which partisan and constituency factors are expressed. Constituency factors are assumed to be constant over the time periods for which the change measures are computed. These are four-year time periods, including the last term of the incumbent and the first term of the freshman member for each constituency in which a partisan turnover has occurred. The assumption that constituency factors are constant refers to an objective level of influence exerted by these factors, with the *perceptions* of the constituency factors treated as a source of variation in the behaviors of the individual representatives.

While the constituency influence is held constant, the partisan influence is indicated by the difference in the policy positions taken by the new member of one party and the incumbent of another party. The implication that follows from the assumption of constancy in the constituency pressures is that the stronger those constituency pressures *relative* to partisan pressures (whatever the specific components of either may be), the smaller the change should be when there is a partisan turnover in the representation of a constituency.

The import of the data analyzed thus far is that the relative influence of party is greater with respect to economic policy as constituency factors have a greater impact on welfare policy voting. However, the previous set of data have not been so definitive in this respect that further analysis is redundant. Therefore, let us return to an examination of the party change (or partisan turnover) ratios presented previously:

House Economic Policy: 8.0 Welfare Policy: 4.0
Senate Economic Policy: 5.8 Welfare Policy: 6.0

The proposition that party is relatively more influential on economic policy voting is supported in the House but not in the Senate; in the House the party change ratio on economic policy is twice that on welfare policy whereas in the Senate the difference is insignificant and contrary to the proposition. However, these institutional differences are so interesting in themselves as to take some of the sting out of the blow administered to the proposition by the Senate.

The low value of the party-change ratio on House welfare policy, compared to the higher ratio value on Senate welfare policy, is consistent with institutional differences. It is understandable that the House member should be more subject to constituency pressures than the Senator in view of the greater level of homogeneity of House constituencies. The House member can respond to relatively clear constituency voting cues. In contrast, the Senator with a more heterogeneous constituency, unable to identify clear constituency instructions concerning his behavior, is less constrained by constituency pressures. In the absence of clear constituency constraints, the higher partisan influence reflected in the party-change score on Senate welfare policy may reflect the necessity of relying more heavily on partisan ideology or partisan positions. Alternatively, in the absence of clear constituency cues, the Senator may feel there is less danger in expressing partisan views as the most appropriate governmental response to constituency demands or needs.

The lower level of the party-change ratio on Senate economic policy, relative to that of the House, may indicate that the Senator is not as tightly bound by partisan influences on a dimension where such influence is uppermost. This is not inconsistent with our observations on welfare policy for we did not suggest there that the Senator was bound tightly by partisan constraints, rather that he had to rely upon them, or had the opportunity to express them, to a greater extent than the House member. Here we are suggesting that the Senator is no more bound by partisan constraints on economic policy than he is by constituency constraints on welfare policy.

House members, on the other hand, may be much less able to withstand partisan pressures on economic policy, or be much less disposed to, in the absence of strong constituency pressures. In the words of Sam Rayburn, the House member has to "get along" if he wants to move into positions of influence or obtain support for his own legislative requests. Nor does the individual House member possess the opportunity of the individual Senator to build a personal public reputation to compensate for his legislative ineffectiveness.

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[19] With one exception, a Democratic Senator replacing a Republican Senator in Ohio, partisan turnover produces a change in the expected direction: a Democratic legislator more liberal than a Republican.

In general, these findings reflect the common view of the free-wheeling, nationally oriented Senator operating in the smaller, more loosely organized body and the cautious, locally oriented Representative "getting along" in the larger, more tightly organized House. It would appear that the House member is more tightly circumscribed in his behavior. On the dimension where party influence is considered to be the greatest, the House member is more responsive to party influence than his Senate colleague; where constituency is considered to be relatively more important, the House member is relatively more subject to constituency constraints. This does not mean that the Senator is not also affected by these constraints, only that he appears to be less so.

## III. SUMMARY

In our opening remarks, the manifest purpose of this roll call analysis was stated to be that of demonstrating the presence of two policy dimensions, economic and welfare policy, in the voting of Senators and Representatives during the 83rd through the 88th Congresses. Evidence in support of this proposition has been presented in accordance with the criteria of a model of measurement identity. It consists of inter-scale correlations in each house, an examination of the content of the roll calls associated with the two dimensions, and correlations of the measures of the two dimensions with constituency characteristics and with party.

It was further proposed that partisan differences would be more relevant to economic than to welfare policy voting. The evidence used here was drawn, in large part, from the evidence used to substantiate the first proportion; this is consistent with the inter-dependence of the two propositions. In addition, a stability and change analysis gave additional support to the proposition in relation to House voting, leaving open the question of its applicability to Senate voting.

Also suggested in the introductory remarks, was the value of utilizing preliminary definitions of roll call policy domains prior to the objective statistical search for voting dimensions. One advantage is that of establishing the bases of longitudinal and comparative analysis in terms of subjectively discriminable policy domains. Our analysis has indicated also that the distinction between highly correlated policy dimensions may become blurred if an objective analysis of roll call interrelations is applied across policy domains. This becomes particularly evident in a longitudinal analysis in which statistical convergence and divergence of policy dimensions have time to appear, so to speak. It is clear that our view of the voting patterns would have been somewhat different if the entire set of welfare and economic policy roll calls in each Congress had been subjected to a common correlational analysis. This is not to claim that the approach we have used is superior; it is to suggest that the choice to be made is neither inconsequential nor self-evident. However, to the individual with an interest in issues, only a moderately high tolerance for abstraction, and some skepticism as to the potential of uncompromising objectivity, the advantages of the policy domain approach may be highly persuasive.

One of the more unique aspects of the present study has been the comparison between Senate and House voting. Firstly, it was observed that House voting is more simply structured than Senate voting. Thus, for both policy domains a larger proportion of the House votes were included within the primary clusters associated with each of the major policy dimensions. This finding was attributed to the greater opportunity for detailed consideration of legislation on the floor of the Senate which, in turn, induces a greater complexity in the dimensional structure of the set of Senatorial voting acts.

Secondly, the analysis has indicated that Senators are less subject to constituency pressures and more independent of partisan constraints. With respect to Coastal Republicans and Southern Democrats, there is also some indication of a higher level of responsiveness to factors associated with a partisan turnover in the White House. This is a particularly interesting area for further analysis because the responsiveness of these two groups to changes in the occupancy of the White House is consistent with the relative absence of mutually reinforcing partisan and constituency referents. In terms of the dominant historical urban-rural, Democratic-Republican parallel, Coastal Republicans and Southern Democrats are political anomalies.

Thirdly, and most importantly, this study has provided substantial evidence of a similarity in the dimensional structure of voting on economic and welfare policy in the Senate and the House. If parallel dimensions emerge in other policy domains, it suggests that Congress may be viewed as a political subsystem in which values are allocated within a common frame of reference. Furthermore, in the absence of evidence that Congress is a subsystem that rapidly socializes its members with regard to modes of cognition and perception, the finding of parallel dimensions in the two houses suggests that these dimensions penetrate the political strata from which Congressmen are recruited. Thus the new member is already accustomed to viewing specific policy questions in terms of more general policy categories.

More generally, with respect to public opinion, it would be of considerable interest to re-examine studies of public opinion in different political strata to determine the penetration of the economic-welfare policy distinction downward from political elites to the political mass. An earlier study has indicated that the personal attitudes of members of the House can be differentiated with respect to welfare and economic policy.[20] What are the limits on the penetration of this policy distinction down through the political strata? Is there sufficient penetration to warrant the conclusion that partisan ideology is involved in the attitudes on economic policy whereas social welfare attitudes have other origins? For example, does the welfare-economic policy distinction carry down through those political strata which icnlude people who are subject to partisan socialization of political attitudes and disappear when that form of political socialization is the weakest? More generally, we are suggesting that studies of legislative voting behavior can be particularly useful in measuring policy dimensions that are relevant for one set of political elites, and possibly relevant for a more inclusive set, with the degree of penetration down through the political strata a matter of theoretical interest, not only in terms of the mechanisms of political socialization, but in terms of the linkages between the electorate and the policymakers.[21]

Related to the penetration phenomenon with respect to dimensions of political attitudes is the comparison of Congressional voting with voting in the state legislative bodies, and other national institutions such as the Supreme Court. In the last instance, Guttman scale and factor analysis of Supreme Court decisions has shown the presence of an economic liberalism dimension which appears to be highly similar to the economic policy dimension in Congressional voting.[22]

Whatever the cite of the public opinion or voting analysis, there is a case to be made for a high degree of sensitivity to the policy component, as opposed to the alignment component, of the analysis. A policy orientation leading to subjective divisions of items into policy domains provides the basis for comparative and longitudinal studies. In addition, without such considerations of policy content, the analyst is likely to become the victim of his objective procedures. Thus, the dimensional structure which is identified is, in part, a function of the criteria used to define clusters, factors or dimensions. The more stringent these requirements become, the greater the complexity of the dimensional structure. Finally, as we have indicated previously, the totally objective analysis is inadequate to cope with the problems involved in the change of positions by individuals over time; this problem is stated most briefly as that of convergence and divergence among policy dimensions.

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[20] Clausen, *op. cit.*, pp. 1031–1033.

[21] Warren E. Miller and Donald E. Stokes, "Constituency Influence in Congress," this REVIEW, 57 (March, 1963), pp. 45–46.

[22] Glendon Schubert, "The 1960–1961 Term of the Supreme Court: A Psychological Analysis," this REVIEW, 26 (March, 1962), 90–107; Harold J. Spaeth, "Warren Court Attitudes Toward Business: The 'B' Scale" in *Judicial Decision Making,* Glendon Schubert (ed.), (London: The Free Press of Glencoe, 1963), pp. 73–110.

# BASES OF SUPPORT FOR MEXICO'S DOMINANT PARTY*

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The purpose of this paper is to elucidate the bases of support for Mexico's *Partido Revolucionário Institucional.* A model is developed which identifies the major and minor variables affecting changes in *PRI* support in the six elections between 1952 and 1967. Throughout the paper the unit of analysis is the state; the dependent variables are voter turnout and the percentage of the total vote in each state received by the *PRI*.

## I. INTRODUCTION

The paucity of empirical works on electoral support for dominant-party regimes seems to indicate difficulties in data collection, not simply a dull topic. The emphasis on institutionalized parties in the works of such scholars as Huntington, Emerson, and Zolberg underlines the need for a better understanding of the roles played by such parties in nations at varying levels of development.[1] Models of electoral support should help us understand the techniques of social control available to elites, to predict changes in the party system, and to assess the effects of governmental outputs. Mexico is particularly interesting because it appears to be contrary to much of the political development literature, which suggests that oppositions are more likely to flourish as politics modernize, though. Yet Mexico is modernizing, the *PRI* so far shows no loss of support. In later sections of this paper I will speculate on the causes of this paradox.

First, however, let me explicate an assumption crucial to this research: that elections in a one-party system are a significant part of the political process. Mexicanists divide on this question, their opinions generally correlating with their estimates of the importance of the governing party in policy-making. Frank Brandenburg, who downgrades the policy influence of the *PRI*, claims that "Elections, whether internal to the party or external and open to the public, are hardly more than travesties."[2] Robert Scott, who emphasizes the importance of the *PRI* in policy-making, claims that the *PRI* desires to maximize its vote, that differences in *PRI* percentages may stem from such variables as economic conditions or proximity to the United States, and that vigorous electoral activities, such as registration drives, reflect the party's use of the election as a barometer of public confidence.[3] The empirical evidence seems to support Scott's position. Not only is there considerable electoral activity, but in some cases the *PRI* has actually suffered defeats.[4]

A broader answer to the question of theoretical relevance requires a consideration of the constraints on political mobility in Mexico. Since the party is highly centralized, gains in political status for middle-level elites depend on a favorable relationship with higher-level elites. Because the *PRI* dislikes evidence of dissatisfaction among the populace, the availability of a clear test such as an election becomes significant to elites aspiring to higher office. The election provides an opportunity to demonstrate political skill in the same sense that a machine politician in the United States tries to "deliver the vote." Hence elections in Mexico gain significance from the desire of elites to maximize support and from their importance in the recruitment process.

In the remainder of this paper we first consider a theoretical framework linking the vari-

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* I am indebted to Bashirrudin Ahmed, John Ferejohn, Richard Fagen, Robert Packenham, Barry Rundquist, and Richard Winters for advice and comments.

[1] Samuel P. Huntington, "Political Development and Political Decay," *World Politics,* XVII (April, 1965), 385–430; Rupert Emerson, *Political Modernization: The Single Party System,* Monograph No. 1, (Denver: University of Denver, 1963–64); Aristide R. Zolberg, *Creating Political Order: The Party States of West Africa,* (Chicago: Rand McNally, 1966).

[2] Frank Brandenburg, *The Making of Modern Mexico,* (Englewood Cliffs: Prentice-Hall, 1964), pp. 223–243.

[3] Robert Scott, *Mexican Government in Transition,* (Urbana: University of Illinois Press, 1964), pp. 223–243.

[4] In the 1958 election the *PRI* lost nine elections for seats in the Chamber of Deputies, and more recently, the mayoralties of the cities of Hermosillo and Mérida. See Scott, *ibid.,* p. 243. Also see Bo Anderson and James Cockcroft, "Control and Cooptation in Mexican Politics," *International Journal of Comparative Sociology,* VII, (March, 1966); and William Tuohy and David Ronfeldt, "Political Control and the Recruitment of Middle-level Elites in Mexico: an example from Agrarian Politics," *Western Political Quarterly,* 22 (June, 1969).

ables in our model. Next, the directions of the relationships between the variables are predicted and the model is empirically tested. In the last sections we assess the validity of the model and attempt to explain the paradoxical nature of *PRI* support.[5]

### II. THEORETICAL FRAMEWORK

When elections are hotly contested, the meaningful dependent variables are likely to be those identifying the winner or his party and measuring his vote total. Since the winner of a Mexican election is seldom in doubt, and because elections in Mexico usually indicate political skill and regime legitimacy, the number of people turning out on election day may be as interesting as the direction of the vote. Evidence for the importance of "getting out the vote" is provided by Scott in his description of voter registration efforts in the 1958 campaign.[6] Feverish registration activity (which in the highly competitive Federal District could well have increased the party's percentage as well as total vote) seems to have been a result of the elite's belief that non-participation in the election is equivalent to apathy and non-support, and that apathy threatens the viability of the Revolutionary Ideology. In this sense turnout is both a "means" and an "end." Because turnout affects the *PRI* vote percentage, it is an independent variable; because the *PRI* acts to promote turnout, I also consider it as a dependent variable.

Factors that contribute to the electoral strength of the *PRI* may be divided into two categories: direct and indirect. By direct factors I mean those that the *PRI* or the government itself may manipulate in the period around the election, such as vigorous campaign activity. By indirect factors I mean those that it can affect only over a long period of time or not at all (definitely not in the years between two elections). Because I wish to avoid giving an aura of precision to an inherently arbitrary categorizing process, this definition has been deliberately left vague.

Table 1 identifies some factors likely to influence elections in any political system. Obviously these have varying degrees of applicability and operationality in the Mexican case. In the next section we consider variables of particular relevance to Mexican elections, hypothesize linkages between independent and dependent variables, and describe the operationalization of each variable.

TABLE 1: A NON-EXHAUSTIVE AND PARTIALLY OVERLAPPING LIST OF DIRECT AND INDIRECT FACTORS AFFECTING TURNOUT AND DIRECTION OF THE VOTE

| | Direct | Indirect |
|---|---|---|
| *Turnout* | Benefits (e.g., welfare payments)<br>Campaign Issues<br>Development Projects<br>Local Leadership<br>Monetary Incentives<br>Number of Party Members<br>Party Activity<br>Registration<br>Registration Rules | Border with United States<br>Climatic Conditions<br>Direct Factors in Previous Elections<br>General Economic Conditions During Election Period<br>Historical Nonintegration<br>Level of Development<br>Long-term Benefits<br>Long-term Strength of Oppositions<br>Turnout in Previous Elections |
| *Direction of Vote* | Benefits<br>Campaign Issues<br>Development Projects<br>Local Leadership<br>Media Support<br>Monetary Incentives<br>Number of Party Members<br>Party Activity<br>Turnout | Border with United States<br>Direction of Vote in Previous Election<br>General Economic Conditions During Election Period<br>Historical Nonintegration<br>Level of Development<br>Long-term Benefits<br>Long-term Strength of Oppositions |

### III. DEPENDENT VARIABLES

*Direction of Vote.* The major dependent variable in this analysis is the percentage of the total vote received by the *PRI* in six elections between 1952 and 1967. Three of these elections, 1952, 1958, and 1964, were Presidential elections; those of 1955, 1961, and 1967 were Congressional elections. For each state I have averaged the percentages received in the six elections. Since the six elections cannot be regarded as six separate samples, the number of cases is 32—the number of states in Mexico—rather than 192.[7]

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[5] I also assume that the reported voting data are meaningful. A variety of error types is possible. Random error is not too important since it tends to lower correlations. A more significant problem stems from the frequent charges of fraud by opposition leaders. If there are inflationary biases in the vote totals, they could take three forms. Perhaps all *PRI* totals are padded by approximately the same amount. This would have the effect of adding a constant and would not seriously affect correlational measures. If the amount added is directly proportional to the percentage of the vote received by the opposition, gross inter-state differences would be reduced, but the rank order would remain the same. If, however, the amount added is related to some unknown variable not of these two types, my results might be seriously affected. At this stage of the research I have no alternative to using reported data.

[6] Scott, *op. cit.*, pp. 225–226.

[7] Election data sources are as follows: 1952,

*Turnout-Direction Linkage.* Voting participation has been identified as a dependent variable of significance in Mexico. Turnout is measured here by the percentage of eligibles voting in each election. Assessing the nature of the turnout-direction linkage requires a knowledge of the predispositions of new voters, since higher participation rates usually mean an influx of previously uninvolved individuals. Scholars are divided on the effects of new voters. Zolberg's work on West African one-party states suggests that high participation helps dominant parties:

> . . . the basic notion that uninvolved individuals respond to the most powerful available stimulus, such as the one provided by a leading personality or an apparently powerful organization is perhaps relatively independent of the cultural context.[8]

A reasonable operationalization of Zolberg's point is as follows: the more a political system is dominated by a single personality or organization, the more the effect of additional voters will be an increase in the strength of the dominant personality or organization. Hence, the *PRI* should be helped by high participation.

The direction of this linkage has been challenged in a study of Mexican elections by Pablo Gonzalez-Casanova. He argues that low turnout helps the *PRI,* because those who vote present the least opposition to the regime. Noting that the poorest states in Mexico vote most strongly for the *PRI,* he contends that rural populations are unorganized and inexperienced in politics, and as a result are passive instruments of their leadership. Being poor and illiterate, their voting rates are lower than those of people in more urbanized states.[9]

Gonzalez-Casanova's position is unclear because it depends on the relationship between participation rates and poverty and urbanization. Since the hypotheses relating participation and direction of vote are contradictory, and since at this point we have no other basis for predicting one way or the other, the turnout-direction linkage will be left open.[10]

## IV. INDEPENDENT VARIABLES

### Indirect Factors

*Level of Development.* Chief among the indirect factors of support is what will be called "level of development." There is a considerable literature arguing that oppositions flourish under conditions of high social mobilization, economic development, urbanization, and "modernization." Blanksten has made this argument for Latin America:

> It is typical of Latin America that, with the exception of the landowners and the church, few interests arising in the rural areas are capable of making themselves heard in politics. In cities, however, interest groups form more readily and give voice to the demands of urbanized sectors of the population. Similarly, new interests find organized expression in consequence of the processes of restratification, secularization, and commercialization.[11]

The effect of urbanization on the opportuni-

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1955, 1958, 1961 are from Pablo Gonzales-Casanova, *La Democracia en México* (Ediciones Era, S.A., 1965), pp. 240, 242–243, 241–247, in that order. The election of 1964 is from *Politica,* (Sept. 15, 1964), p. xix. The election of 1967 is from *El Universal,* July 11, 1967, p. 9. The last election data are unofficial, but changes were very minor. Objection may be made to the combining of Presidential and Congressional election. While the fit of the model does vary among the elections, the basic model is not seriously affected.

[8] Zolberg, *op. cit.,* p. 20.

[9] Gonzalez-Casanova, *op. cit.,* pp. 106–107.

[10] Turnout has been operationalized as the six-election average of the percentage of eligible voters who actually voted. Mexico does have a registration process, and there are differences between the percentage of the population registered and the percentage actually voting. But registration figures for all elections were unavailable, so registration had to be ignored. Turnout is defined as the percentage of the population legally eligible—irrespective of registration—who voted.

Turnout rates were calculated as follows: the actual number of people who voted in each state was computed from the election results (see footnote 7). The percentage of the population eligible to vote was arithmetically extrapolated from 1950 and 1960 population per state on the basis of each state's growth rate from 1950 to 1960 (these differ widely). For the 1952 election males over 20 were used as the base. In succeeding elections both sexes were used. There are two sources of error: arithmetic rather than geometric progressions, and the assumption of state equality in age distributions. The latter error may be significant but is unavoidable. The population figures come from the *Anuário Estadística de los Estados Unidos Mexicanos* (Secretaria de Industria y Comercio, Direction General de Estadística, Mexico, 1965). (for 1962–63), p. 28.

[11] George Blanksten, "The Politics of Latin America," in Almond and Coleman (eds.), *The Politics of the Developing Areas,* (Princeton: Princeton University Press, 1960), p. 477.

ties for oppositions has been explored by Ronald Ridker:

> Costs to political agitators are lower in the city than in the country. The greater concentration of people, the propinquity to centers of political decision-making, and the greater degree of literacy in cities, also may result in a higher level of aspiration. In addition, urban life generates unrest, particularly for the new immigrant, because of the greater disparity in the ways of life of cities and farms . . . urban populations tend to be very responsive elements for radical propagandists.[12]

This does not mean that only leftist opposition activity is likely. The kind of competing-elites problem discussed by Shils and Eisenstadt increases the chances of opposition from either left or right.[13] The fact that urban areas have a higher proportion of middle-class residents is emphasized by Lenski, who finds "a reaction to the exercise of power and privilege . . . in the efforts of members of the middle classes to gain control over powers, privileges, and resources traditionally reserved to the elite."[14] That the electoral arena is a natural one for this contest is suggested by D'Antonio and Form's discussion of the most important opposition party, the National Action Party.

> . . . its voting support has come from white collar workers, serious churchgoers, some small businessmen, and employees of the *PAN* activists.[15]

Urbanization is usually coupled with economic growth, which in itself can be a source of opposition. Mancur Olson stresses the diverse nature of the dissatisfaction that may arise in conditions of rapid growth:

> Both gainers and losers from economic growth can be destabilizing forces . . . economic growth increases the number of *nouveaux riches,* who may use their economic power to change the social and political order in their own interest; . . . economic growth may paradoxically also create a surprisingly large number of *"nouveaux pauvres,"* who will be much more resentful of their poverty than those who have known nothing else.[16]

In addition, particular traditional factors may discourage opposition in less developed areas. L. Vincent Padgett argues that the norms of unanimity in decision-making and suspicion of outsiders combine to cause village Mexicans to protect rather than mobilize against the local boss. He further contends that "The farther from the effects of modernization, including education and transportation, the more dependent the people are upon their *caciques.*"[17]

The above scholars are generally agreed that conditions in urban areas are more favorable to the development of oppositions.[18] Thus we suggest that the higher the level of development in a state, the lower will be its *PRI* vote.

The effect of development on turnout is less clear. Daniel Lerner emphasizes urbanization as the prime mover in the process of modernization. He argues that:

> . . . increasing urbanization has tended to increase media exposure; increasing media has "gone with" wider economic participation (per capita income) and political participation (voting).[19]

Lerner's analysis is based on the assumption that the effects of the urbanization process ought to be most visible in the most urban areas, unless these areas have had long-static large populations. A subnational application of Lerner's argument leads us to expect high turnout rates in the more developed areas. Gonzalez-Casanova made essentially the same point in examining Mexican elections, but no supporting data was presented.[20]

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[12] Ronald Ridker, "Discontent and Economic Growth," *Economic Growth and Economic Change* XI (October, 1962), p. 11. Some of Ridker's argument is certainly debatable, especially in regard to new immigrants, but the essential point seems valid. See Wayne A. Cornelius, Jr., "Urbanization as an Agent in Latin American Political Instability: The Case of Mexico," this REVIEW, 63 (Sept. 1969).

[13] Edward Shils, *Political Development in the New States,* (London: Mouton, 1965); and S. N. Eisenstadt, "Breakdowns of Modernization," *Economic Development and Cultural Change,* 12, pp. 345–367.

[14] Gerhard Lenski, *Power and Privilege,* (New York: McGraw Hill, 1966), p. 65.

[15] William D'Antonio and William Form, *Influentials in Two Border Cities,* (South Bend: University of Notre Dame Press, 1965), p. 37–38.

[16] Mancur Olson, "Rapid Economic Growth as a Destabilizing Force," *Journal of Economic History,* (Dec., 1963), p. 533.

[17] L. Vincent Padgett, *The Mexican Political System,* (Boston: Houghton Mifflin, 1966), p. 83 fn. Padgett's emphasis on the special characteristics of less-developed areas reinforces the notion that absolute levels of development may be more important than the relative positions of two or more states. For example, the difference between the levels of urbanization of Idaho and California, and Tlaxcala and the Federal District (Mexico City) may be equal; what is more relevant is the absolute position of all these places on a traditional-modern continuum.

[18] There is also some inconclusive evidence suggesting a relationship between urbanization and party competition in the United States. For a review of this literature, see Philip Coulter and Glen Gordon, "Urbanization and Party Competition," *Western Political Quarterly,* XXI (June, 1968), 274–288.

[19] Daniel Lerner, *The Passing of Traditional Society,* (Glencoe: Free Press, 1958), p. 46.

Evidently the association between development and participation is not universal. Steiner found that the norm of unanimity in decision-making, present in Japanese rural socio-economic units (*buraku*), tended to *increase* voting levels.[21] And Dupeux found that in France, because of the higher average age of rural voters, the training citizens get in local politics, and various historical reasons, participation was highest in rural areas.[22]

Since there is conflict in the literature, the direction of the linkage between level of development and turnout will be left unspecified at this point.

As an indicator of level of development I have used urbanization.[23] It is defined as the percentage of the population in each state living in communities of more than 2500 people.[24]

*Historical Nonintegration.* Certain states in Mexico have traditions of isolation and nonintegration vis-à-vis the national political and economic system. Four states seem particularly separatist. Yucatán has a long tradition of separatism and localism. Until 1950 the Yucatán peninsula had no unbroken land tie with the rest of Mexico, and the inhabitants once declared independence. Yucatán's problems were worsened by the failure of a government sponsored henequen growing cooperative in 1954.[25]

A second state with a separatist tradition is Oaxaca, where citizens as late as 1952 held mass meetings asserting their desire for local autonomy. Although the situation is now improved, Scott claims that Oaxaca remains one of the least integrated areas in Mexico.[26]

The Baja California region does not suffer from the poverty of the aforementioned areas, but it is geographically separated from the main land area. The northern part recently become a state, but both it and the territory of Baja California are still rather isolated.[27]

From the foregoing discussion the direction of linkages between nonintegration and both turnout and direction of vote should be clear. I hypothesize that states (or territories) with such tendencies ought to have lower rates of turnout and lower percentages for the *PRI*.[28]

*Presence on the United States Border.* Political systems do not exist in vacuums. A Mexican state is part of a system including other states, the Federal Government, and political actors of other national units. The political environment of states along the United States-Mexico border surely differs from that of states bordering only on Mexican states; hence, it is not surprising that politics in these states should be affected by proximity to the United States.

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[20] Gonzalez-Casanova, *op. cit.*, p. 106–107.

[21] Kurt Steiner, Stanford University, unpublished paper, 1966. Similar findings about Malaya are presented by Richard Winters, Dartmouth College, in an unpublished paper.

[22] Georges Dupeux, "France," *International Social Science Journal,* XII (No. 1, 1960), pp. 46–47.

[23] Objection may be raised to the use of this indicator of level of development. First many scholars cited in the above theoretical section explicitly use urbanization as their variable. Second, as Schnore found, urbanization is highly related to many alternative indicators. He concluded that "Urbanization is an intrinsic part of modernization in general." (See Schnore, "The Statistical Measurement of Urbanization and Economic Development," *Land Economics,* 38, 1961. I found, using Mexican data, that a variety of independent variables are all intercorrelated averaging .80.) Third, data for some alternative indicators, like GNP, is unavailable. As to the use of absolute levels rather than rates of change, we rely partly on Raymond Tanter's argument: "Although most of the hypotheses above are stated temporally, implying the need for a change measure of urbanization, empirical research suggests that the level and rate of urbanization operate similarly vis-à-vis other processes such as measures of social mobilization:" Raymond Tanter, "Toward a Theory of Conflict Behavior in Latin America," (University of Michigan, unpublished paper, 1967), p. 8. Although I believe that the "true" causal variable is a composite of many indicators of level of development, the variable will hereafter be specified as "urbanization."

[24] James Wilkie, *The Mexican Revolution* (Berkeley: University of California Press, 1967), pp. 218–219. A square root transformation was performed on the variable.

[25] Scott, *op. cit.*, p. 41.

[26] *Ibid.*, p. 40.

[27] *Ibid.*, p. 39.

[28] Since historical nonintegration has no easily measurable attributes, a rough test of the hypotheses could be made by testing the adequacy of the model without the variable. Such a procedure, however, fails to reveal interactions between this variable and other variables in the model. My procedure instead will be to construct a dichotomous (dummy) variable which will be allowed to enter the equations freely. Hence I coded Oaxaca, Yucatán, Baja California (Norte) and Baja California Territory as "1" and all the other states as "0" for this variable.

In their study of the border cities of El Paso, Texas, and Ciudad Juarez, Chihuahua, D'Antonio and Form discuss the effects of proximity to a large Texas city. They suggest that:

> the long common history of the two cities may have produced an overriding border culture which contained certain structural ambiguities and anomalies not ordinarily found in cities clearly embedded in a distinct culture.[29]

The effects of living close to the United States are particularly strong on Mexican influentials:

> To the Mexican influentials, especially the businessmen, the border represented more than economic opportunity . . . they looked to the United States as a source of values, ideas, and ideals. A majority had studied in American schools, learned English, observed how the American system worked, and made some strong friends in the States.[30]

The *PRI* faced considerable opposition in Juarez during the 1950's from the *PAN*. The strength of the opposition seemed based on the vigorous associational interest groups dominated by business influentials. Such interest groups paralleled similar groups in El Paso. Businessmen's images of the political and economic system of El Paso may have helped invigorate associational interest groups and legitimize opposition. Therefore I hypothesize that states bordering on the United States will have higher levels of participation and lower levels of *PRI* vote.[31]

*Structure of Opposition.* It might be argued that the use of *PRI* vote as the dependent variable implies that Mexico has a two-party system; i.e., the *PRI* versus all other parties. Though opposition parties on the left and on the right draw voters from different sectors of the electorate and have different significance for the *PRI* leaders, I suggest that lumping together all oppositions is justified as long as explanations are directed to conditions favorable to high *PRI* votes rather than to conditions favorable to high opposition votes.

Although the above position is defensible, at this point certain aspects of the opposition are related to the *PRI* vote. In particular, I am interested in discovering a relationship between the number of parties in opposition to the *PRI* and turnout and direction of vote. In Presidential elections usually only the *PRI* and the *PAN* put up candidates, but in off-year elections there may be as many as four minor parties running candidates for the Chamber of Deputies. Since these parties would probably not put up candidates if there were no potential voters, their presence ought to have the effect of increasing turnout—though perhaps only slightly, since the majority of their voters could be people who are drawn from the established parties.

The effect of oppositions on the direction of the vote is less clear. Splinter parties may attract persons who were previously nonvoters; thereby reducing the *PRI* percentage. But these minor parties might direct attacks against each other rather than against the *PRI*. In addition, the fragmentation of the opposition might confuse potentially anti-*PRI* voters and result in withdrawal. These latter possibilities would increase the *PRI* percentage. Predicting the net effect of these counter-trends is impossible without more information about the parties, so this link will be left unspecified.[32]

*Other Possible Variables.* My selection of variables has been determined partly by theoretical relevance and partly by data availability. There are a variety of other indirect factors of obvious relevance; hence, this discussion is not meant to imply theoretical closure.[33]

### Direct Factors

*Party Activity.* The most natural means by which a party can influence an election are the myriad activities falling under the general rubric of "party activity." In Mexico these might include rallies, get-out-the-vote drives, propaganda, canvassing, etc. Unfortunately, none of these variables can be quantified given the present state of knowledge about Mexican politics. Hence, this study has no measures of party activity; the following variable definitely should not be construed as such a measure.

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[29] D'Antonio and Form, *op. cit.*, p. 218.

[30] *Ibid.*, p. 219.

[31] Six states border on the United States: Baja California, Sonora, Chihuahua, Coahila, Nuevo Leon, and Tamaulipas. Using the same technique as with the previous variable, these states are coded "1", all other states are coded "0".

[32] This variable has been operationalized by counting all non-*PRI* parties that received any votes in 1955, 1961, and 1967, and summing the three totals. The scores range from three to ten. A square root transformation was performed on this data.

[33] Among other useful measures: economic conditions at the time of the election, especially cost-of-living indices and unemployment rates; climatic conditions on election day; and what some American scholars euphemistically call "Deaths from civil violence;" i.e., dead students. Quantification of these variables requires more data.

*Affiliated Party Members.* Rigorous interpretations of party membership are slippery because of the variable nature of formal membership. Although certain groups (like the military) cannot formally organize within the party, anyone may join. In addition, members of certain occupations are automatically counted as *PRI* members. For example, in a study of Xalapa, the capital of Veracruz, William Tuohy found that the *PRI's* published membership figure was equal to about 75 percent of the adult population. Tuohy explains this exaggeration by noting that all government employees and union members were counted as well as some persons below voting age. Of the reported total, only a few paid dues and actively participated. A sample survey in the same community found that about five percent of the adult population reported being *PRI* members.[34]

What are the effects of high percentages of the population (inflated or not) affiliated with the *PRI?* Party members may be the basis of party activities; i.e., there may be a high correlation between *PRI* membership in a state and total party activity in the state. *PRI* members may also increase turnout and vote by encouraging non-members to participate. Perhaps their major influence is on their own vote. As a result of being enmeshed in the *PRI* communications network, or because the same predisposition that led to joining the party leads toward a *PRI* vote, members may vote for the *PRI* more often than non-members in similar age and socio-economic groups. Given the level of analysis of our data, accepting any of these competing explanations would risk the ecological fallacy, but we can predict that turnout and *PRI* vote will be higher in states with high percentages of the population affiliated with the *PRI.*[35]

*Benefits and PRI Organization.* The suggestion that governments may use their distributive capability to increase popular support is hardly novel. Particularly when the government also has a considerable symbolic capability (manifested in Mexico, for example, in the association of the party and the Revolutionary Ideology, and disseminated by government-dominated media), it may try to direct affective response to an expenditure such as a new school into a large affirmative vote for the regime. And while such benefits may have long-run destabilizing effects resulting from their contribution to a state's level of development, their short-run effect is likely to be positive for the *PRI.*

To assume, however, that the impact of governmental benefits can be assessed by considering only their effects on the recipients, and to assume that such benefits are both perceived and rewarded by the voters, would be considerable oversimplification. Mediating between the outputs of the national government and the individual citizen is the local or state *PRI* organization. This mediating position stems from two considerations. First, the impact of governmental expenditures may be much greater on the organization than on the citizen, because the party is more likely to be aware of governmental benefits than the citizen, and because the citizen may become aware of the benefits only through the party. Second, an organization so strengthened by the government may be better able to mobilize the electorate in succeeding elections.

The simple benefits-citizens link may have partial validity, but we can suggest a variety of more complicated linkages which include the *PRI* organization as mediator. For example, strong state *PRI* organizations (or state strongmen) may be able to force the national government to supply benefits to the people of a state, who then respond by rewarding the government in national elections and by supporting the state organization electorally, financially, and obediently. The strengthened state organization is then better able to mobilize the electorate, further increasing support, and also to wrest more benefits from the national government. In this case the populace is made aware of the benefits by the state party, which endeavors to present them with a "courtesy of your friendly state *PRI* organization" sign.

The outputs of the national government may be imperceptible to the population but may still have an effect. If their distribution is controlled by the state organization, benefits may be translated into jobs in or dependent upon the party. This strengthens the organization and once again improves its ability to mobilize the population to turn out and support the *PRI.*

In order to frame hypotheses specifying the direction and size of government benefits, we first must consider possible allocation policies for the national government in relation to state *PRI* organizations of differing strength. Here the problem of measuring the strength of the state organization arises. It seems impossible to find any measure that is not based either on the electoral success of the party or on its membership. Electoral success is our dependent variable; the same variable cannot be both the means and the end of our explanation unless we have a time-series analysis. And there are other aspects of organizational strength besides electoral success in a dominant-party system, such as control over participation channels.

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[34] William Tuohy, "Institutionalized Revolution in a Mexican City: Political Decision-Making in Xalapa," unpublished Ph.D. dissertation, Stanford University, (August, 1967), p. 27. The sample survey was conducted by Professor Richard Fagen of Stanford University.

[35] Gonzalez-Casanova, *op. cit.,* p. 238. A logarithmic transformation was performed. The number of officially reported *PRI* members was available only for 1962. Census data was arithmetically extrapolated to give a population base.

In the case of membership, we know that the *PRI* figures are exaggerated; moreover, the number of party members, like electoral success, is not a good indicator of the strength of the organization. (Party membership is utilized as a variable elsewhere in this analysis not as an indicator of party strength but because the number of *PRI* members, regardless of their committment or activity, ought to affect the *PRI* vote.)

It was finally decided not to consider party strength. In this case it seems better to admit that more research is needed than to use an untrustworthy indicator. Thus in this analysis the reality of the party organization as a mediator between the national government and the population of a state has to be ignored.[36]

The loss of this variable in this analysis is not devastating, however, because we can still frame hypotheses on the assumption that at least some benefits are perceived by some citizens, and that the *PRI* is interested in maximizing the effects of the simple benefit-voter linkage.

Predicting the direction of the benefit-voter link is not difficult; we expect benefits to lead to higher turnout and higher *PRI* percentages. But because the bulk of our analysis is done not with a time-series but with an average of the six elections, certain cautions are necessary. For example, if we measured benefits and electoral results in the same short time period, and if we discovered empirically that the *PRI* percentage was lower in areas receiving high benefits, it could be argued that the benefits caused disaffection and lower support for the *PRI, or* that the *PRI* is spending in areas where it is weak, but the benefits have not yet had time to be effective. This kind of problem can be solved definitely only by time-series analyses (which are briefly pursued below); it is partially alleviated by selecting a measure of benefits which predates the elections period sufficiently so that the benefits have had time to effect citizen perceptions.

Another problem with the benefit-voter hypothesis is the operationalization of the variable itself. Since we are concerned with the effects of benefits on people and with the ability of a government to manipulate such benefits, we have to consider the parameters governing population response and speculate on "rational" allocation policies for a national government desirous of maximizing its impact on people in different economic environments.

One possible assumption is that an individual's response to a school, water system, or road is directly proportional to his need for such benefits. Aggregated to the state level this means that a poorer state will reward the *PRI* more than a wealthy state for an equal level of benefits. As a rough test of this hypothesis we can compare the percentage of people in each state benefitted by drinkable water programs between 1946 and 1963 to the state's level of poverty.[37] If in fact the government spent its money in this manner, I would predict higher levels of turnout and *PRI* vote for states with higher benefit/poverty ratios.

The above hypothesis is based on the precarious assumption that people in all social classes are equally likely to perceive government as personally relevant, and that the only factor affecting their estimation of their needs is their objective living standards. The first assumption is certainly wrong; Almond and Verba, among others, have demonstrated cross-nationally the relationship between high social class and perception of the relevance of government.[38] Perhaps poverty is irrelevant as a shaper of demands; everyone wants more government services regardless of his economic level. A "rational" national government might concentrate its benefits in states with larger populations, since (density being equal) more people would perceive the same benefit, and a large state contributes more voters in a national election. To test this reasoning we compare the percentage of the population in each state benefitted by drinkable-water programs to the state's population. If the relationship is positive, then we predict that states with high benefit/population ratios will have higher rates of turnout and larger percentages for the *PRI*.

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[36] Some speculation on "rational" allocation policies is worthwhile. In a two-party system with high party discipline the members of the executive's party should receive the lion's share of patronage, because there is nothing to gain by helping opposition party members. When party discipline is weaker, as in the United States, influential opposition leaders may be wooed, but opposition members in marginal districts would only be strengthened by receiving government benefits. In a dominant-party system we would expect the strong party organizations to struggle for larger shares of outputs. But the government might find it more rational to help the weaker party organizations in order to strengthen them. The ability of the government to pursue such a strategy depends on the alternatives open to the stronger organizations. Is the national party (and hence the government) dependent on the state organizations; do the state organizations have any plausible sanctions, etc.?

[37] James Wilkie, *op. cit.*, p. 217, 248. I recognize that among the imperfections of this and the previous measure are the non-coincidence of the time spans. This will be part of the analysis of time-dependent problems.

Thus far we have discussed a hypothesis based on poverty as a response-conditioner and one which assumes that economic condition is irrelevant to response. A third possibility is that wealth may be a stimulus: i.e., that wealthier areas need and/or demand greater government attention and hence might reward the government for benefits. This kind of thinking is common in economics; Frederick Pryor, for example, lists urbanization and industrialization as variables positively affecting demand for public consumption expenditures.[39] Many types of benefits, such as elaborate sewage systems, are relevant only in urban areas. Hence the level of urbanization can be used as an index of the need for services, and the percentage of people receiving a benefit, in this case drinkable-water programs, is a measure of supply. If there is a positive relationship between the level of urbanization of a state and the percentage of people benefitted, then we would suggest that the government is following this strategy. The relationship between the measures of need and supply for each state indicates the degree of "favoritism" shown the state. We predict that the more a state has been favored by the *PRI*, the higher will be its turnout and *PRI* percentage.[40]

In summary, the concept of benefits is operationalized by considering possible allocation policies for a government desirous of maximizing its impact in terms of electoral support. We cannot choose on "logical" grounds between the three possible expenditure patterns suggested here, because we want to discover what the Mexican government has actually done, not what it would do if its leaders read the *APSR*. Hence the usual procedure in regression studies—throwing all possible hypotheses into the computer and calling the survivors "true" and the fatalities "false"—is inadequate; first, because the theoretical system described here is far from complete; and second, because too many relationships are problematic. Only if we determine in advance what benefit allocation policy the government actually pursued will we be able to make any statements about the effects of benefits on voters.[41]

Thus we want to discover which of these three hypotheses—that benefits in Mexico are distributed to states with the highest levels of poverty, to those with the largest populations, or to those with the highest levels of urbanization—most nearly approximates reality. Empirically we can select between them by comparing the strength of the relationship between the measure of benefits; the percentage of the population benefitted by drinkable water programs, and the three possible attributes of states as recipients: level of poverty, population, and level of urbanization.[42] The results are quite unambiguous. There is no relationship between benefits and the level of population; the relationship between benefits and poverty is moderate and negative, and the relationship between benefits and urbanization is strongly positive. (The Pearson coefficient between urbanization and benefits is .82.)

Hence the *PRI* distributes benefits to states with high levels of urbanization. I predict that the more a state has been favored; i.e., the greater the excess of supply over demands and needs, the higher will be its turnout and *PRI* vote.

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[38] Gabriel Almond and Sidney Verba, *The Civic Culture*, (Princeton: Princeton University Press, 1963).

[39] Frederick Pryor, *Public Expenditure in Communist and Capitalist Nations*, (Homewood: Richard Irwin, Inc., 1968), p. 54.

[40] The actual independent variables are the residuals of a regression between urbanization and the percentage of the population benefitting from drinkable-water programs.

[41] Suppose, for example, that of our three hypotheses, one was found to be associated with an increase in *PRI* vote, one had no effect on the vote, and the third was associated with a decrease in vote. The only conclusion we could draw from such a finding is that if the government followed the first policy, its vote might have increased, but if it followed some other policy, its vote either was unaffected or dropped. These hypotheses are mutually exclusive; we cannot conclude that government policy is "a little bit of everything."

[42] The results are the same if capital spending between 1958 and 1963 is used as the measure of benefits. The actual hypothesis testing was done by comparing the standardized slopes, or Beta coefficients.

At this point I have predicted relationships between each of the six independent variables and two dependent variables. Table 2 summarizes these predictions:

TABLE 2: SUMMARY OF HYPOTHESES

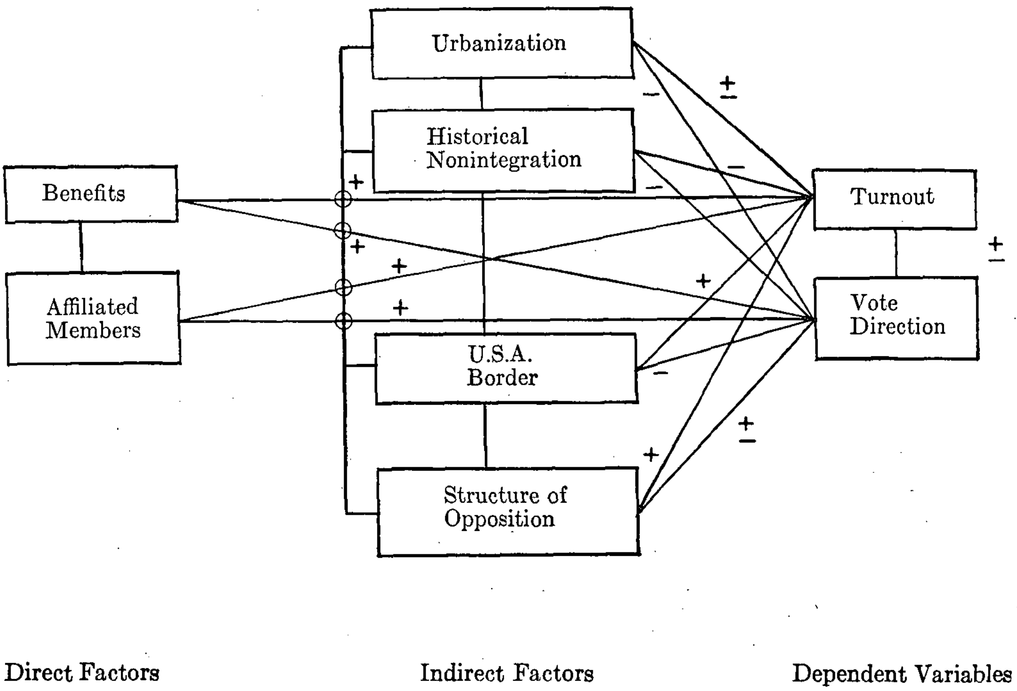
| Independent Variables | Effect of Independent Variable on: | |
|---|---|---|
| | Turnout | PRI Percentage |
| Urbanization | unspecified | decrease |
| Historical Nonintegration | decrease | decrease |
| Presence on USA Border | increase | decrease |
| Structure of Opposition | increase | unspecified |
| Party Membership | increase | increase |
| Benefits | increase | increase |
| Turnout | — | unspecified |

Of course, these variables are not independent of each other, and to begin talking about causality they must be interrelated to separate the independent effect of each from those that are purely spurious. A possible schematic representation of the model is shown in Figure 1.[43]

## V. ANALYSIS

We have thus far developed two sets of prediction equations: one in which turnout is the dependent variable, and a second in which turnout is an independent variable and the *PRI* percentage of the vote is the dependent variable. These hypotheses can be evaluated with multiple regression analyses, enabling us to simultaneously assess the contribution to the dependent variable made by each of our independent variables; i.e., the extent to which unit changes in each of the independent variables are related to unit changes in the dependent variables.[44]

FIGURE 1: A Schematic Representation of the Model



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[43] In this diagram all possible arrows are included. The signs refer to the direction of the predicted relationships. Double signs refer to directions that are uncertain. Circled intersections between the direct and non-direct factors indicate that the effect of each direct factor is measured controlling for all non-direct factors.

[44] The correlation matrix refers to the following variables:

1. The percentage of the population living in communities over 2500.

Table 3 presents the standardized regression coefficients associated with each variable in the two models. The coefficients are statistically comparable so that the relative contribution of each variable can be assessed. (For example, if one coefficient is twice the size of another, the first has twice the explanatory "weight" of the second.) The strength of the variables taken together can be measured by the coefficient of determination ($R^2$), which measures the percentage of the total variance explained by the independent variables.[45]

A number of general substantive conclusions can be drawn from the multiple regression. The model predicting the direction of vote is extremely successful, explaining 75 percent of the variance in the *PRI* percentage. The relatively poor showing of the turnout model is not surprising: it has fewer independent variables, our theoretical underpinning is somewhat shaky, and we lack any reliable measure of party activity. The most significant factor in explaining *PRI* vote is urbanization. Low levels of urbanization are associated with high *PRI* percentage. Evidently Zolberg, Ridker, and Olson, *et al.,* are correct in arguing that oppositions flourish under conditions of high social mobilization; the degree of control exerted by local *caciques* is sufficient to bring out a large *PRI* vote. Figure 2 shows the relationship between urbanization and *PRI* vote for the 32 Mexican states.

The second most important variable is the structure of the opposition. Operationalized as the number of opposition parties, it is associated with low *PRI* vote and also low turnout. Two explanations can be given for each phenomenon. Fragmentation of opposition may *occur* in areas where the *PRI* vote and turnout are lowest, because potential oppositions perceive a receptive public for their efforts. Fragmentation may also have the *effect* of causing *PRI* vote and turnout to decline through the kinds of causal chains discussed earlier; i.e., urbanization leading to new oppositions leading to defeats for the *PRI*.

I originally suggested that a large number of opposition parties would increase turnout, because each opposition party would have its own clientele. Perhaps the error in this reasoning was an overestimation of the salience of party to Mexican voters. New splinter parties may have no clientele of their own; they may draw voters from the major parties but appeal very little to non-voters.

TABLE 3: STANDARDIZED MULTIPLE REGRESSION COEFFICIENTS ASSOCIATED WITH TURNOUT AND DIRECTION OF VOTE

| Independent Variables | Dependent Variables | |
|---|---|---|
| | Turnout | Direction of Vote |
| Urbanization | −.055 | −.579** |
| Historical Nonintegration | .320** | −.238** |
| Presence on US Border | −.113 | .045 |
| Structure of the Opposition | −.305* | −.418** |
| Affiliated Members | .161 | −.174 |
| Benefits | −.085 | −.080 |
| Turnout | — | .276** |
| Multiple Correlation Coefficient | .531 | .880 |
| Coefficient of Determination | .282 | .774 |

* Significant at the .10 level.
** Significant at the .05 level.

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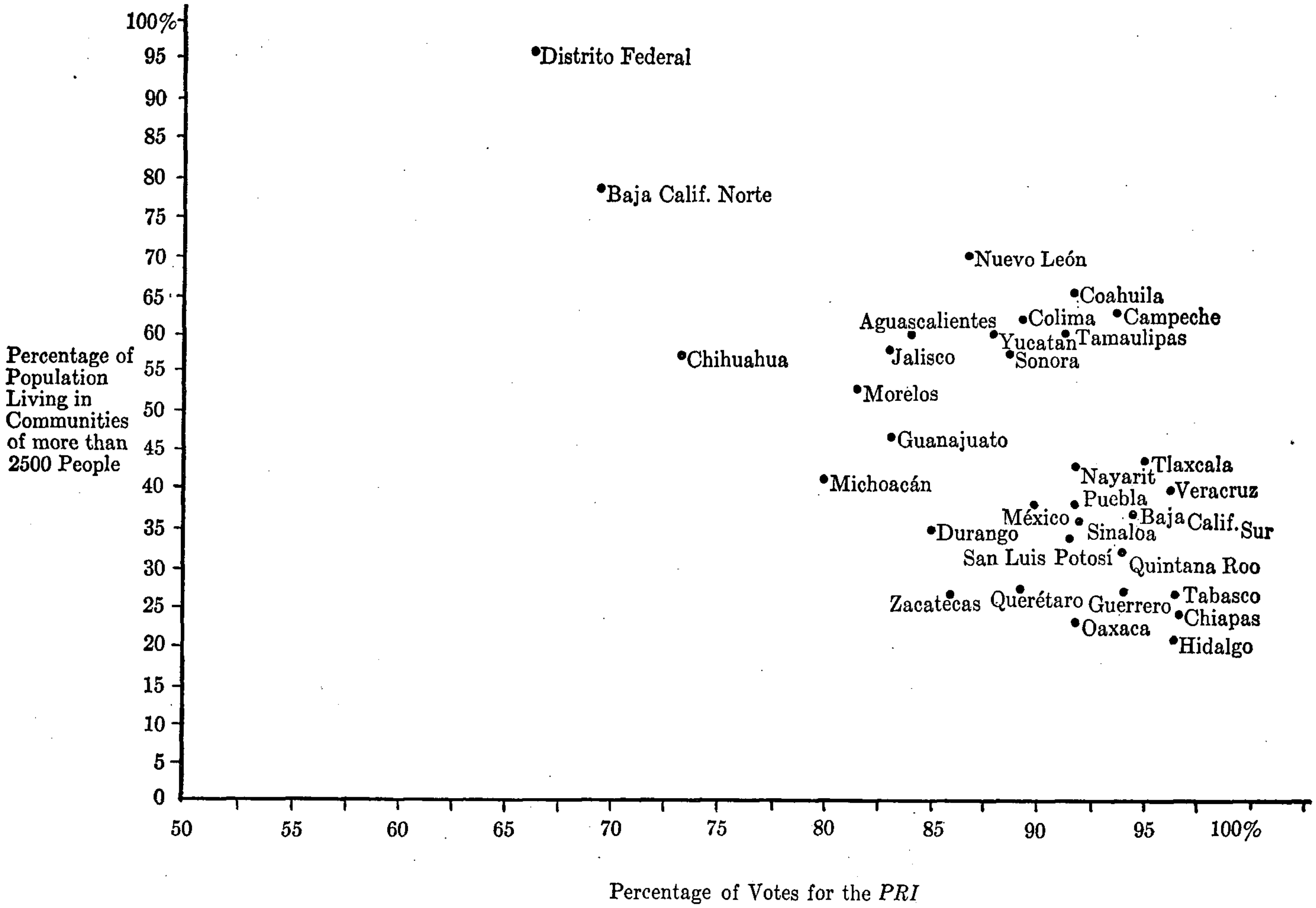
2. A history of nonintegration and separatism in a state.
3. Location of state on border with United States.
4. The number of opposition parties in elections for Congress.
5. The percentage of the population affiliated with the *PRI.*
6. The percentage of the population benefitted by drinkable-water programs.
7. The favoritism of a state over its level of demands for benefits.
8. The percentage of probable electors voting in six elections.
9. The percentage of the vote received by the *PRI* in six elections, 1952–67.

| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 |
|---|---|---|---|---|---|---|---|---|---|
| 1 | — | −.02 | .44 | .15 | .28 | .82 | .00** | −.12 | −.69 |
| 2 | | — | .02 | .01 | −.03 | .00 | .03 | .31 | −.14 |
| 3 | | — | — | .02 | .13 | .46 | .17 | −.13 | −.29 |
| 4 | | | | — | −.50 | .12 | .00 | −.39 | −.53 |
| 5 | | | | | — | .43 | .36 | .25 | −.07 |
| 6 | | | | | | — | .57 | −.11 | −.65 |
| 7 | | | | | | | — | −.03 | −.15 |
| 8 | | | | | | | | — | −.38 |
| 9 | | | | | | | | | |

** The lack of correlation between urbanization and the measure of favoritism does not mean that more urban states are not really getting more than rural states, but only that the relationship is linear; i.e., the tendency in favor of urban areas does not increase with urbanization.

[45] Levels of significance are not being used to test hypotheses in this paper but only to indicate the strength of relationships, because we have a universe of all cases rather than a sample. No relationships are assumed to occur by chance; both weak and strong correlations are theoretically significant. The computational analysis was done on the IBM 360/67 with the Stanford Statistical Package for the Social Sciences.

FIGURE 2


Percentage of Population Living in Communities of more than 2500 People
100%
95
90
85
80
75
70
65
60
55
50
45
40
35
30
25
20
15
10
5
0
50
55
60
65
70
75
80
85
90
95
100%
Percentage of Votes for the PRI
Distrito Federal
Baja Calif. Norte
Nuevo León
Coahuila
Aguascalientes
Colima
Campeche
Yucatan
Tamaulipas
Chihuahua
Jalisco
Sonora
Morelos
Guanajuato
Tlaxcala
Nayarit
Michoacán
Veracruz
Puebla
México
Baja Calif. Sur
Durango
Sinaloa
San Luis Potosí
Quintana Roo
Zacatecas
Querétaro
Guerrero
Tabasco
Chiapas
Oaxaca
Hidalgo

The two remaining indirect factors were entered as dichotomous variables. The effect of presence on the US border is nil. Since the border states are highly urbanized, it might be thought that the effects of presence on the US border are masked by the more powerful variable of urbanization. However an analysis of residuals (produced from a regression without this variable) shows that four of the six states on the US border have *higher* actual *PRI* vote than would be predicted by their level of urbanization. Thus, although the demonstration effect of the United States may carry over into the border *cities*, it does not significantly affect the total vote of the state.

I found that a condition of historical nonintegration lowered the *PRI* vote but increased turnout. The first finding was expected, but the latter is surprising. It may be that oppositions based on localist sentiment are more salient to peripheral participants than those based on ideology; and hence oppositions in the separatist areas stimulate turnout while those in integrated areas depress it. Obviously further research is needed here.

The two direct factors proved weak. In the case of party members the best explanation is probably that the method of determining membership is totally unrelated to party activity. In the case of the measure of benefits it is difficult to reach any conclusion. One possibility is that voters punish but do not reward; i.e., if the level of benefits is above their demand level (as measured by urbanization) they either might not notice the bonus, or might consider it as their due, especially because the need for government services in underdeveloped countries seems infinite. A second possibility is that water programs are a bad measure of benefits. This is suggested by the fact that the states of Michoacán, Puebla and Veracruz are all relatively disfavored. These are the home states of Lázaro Cárdenas, Avila Camacho, and both Alemán and Ruiz Cortines, all former Mexican Presidents. Probably a measure of benefits that includes more types of outputs would not show these states as disfavored. A third possibility is that measurement of the effects of such spending requires disaggregation of the six elections. This will be considered in the next section.

Turnout, the measure of participation, was positively and significantly related to *PRI* vote, supporting our contention that newly politicized voters respond to the *PRI* as the most powerful stimulus. Urbanization is unrelated to turnout. This link may be operable through the mediating effects of a valid indicator of party activity, or it may be that both hypothetically causal forces are at work. In the urban areas turnout is pushed by the kind of spontaneous forces discussed by Lerner. In rural areas, the *PRI* can overcome lack of information and education by working through local *caciques*. Hence the level of turnout could be independent of urbanization even though both forces are operative.[46]

## VI. TIME-SERIES ANALYSES

One purpose of this study is to clarify changes in support for the *PRI* during the elections studied. The introduction posed certain questions answerable only with a trend or disaggregated analysis. One concerns the relation between urbanization rates and *PRI* vote over time; another concerns the relationship between benefits and *PRI* vote. However the dependent variable is operationalized several conclusions are relevant.[47]

First, despite the increasing level of urbanization of Mexico, the *PRI* has become more rather than less dominant. Between 1952 and 1967 the percentage *PRI* rose in every state but three. The average increase was more than eleven percent.

Second, those states with the lowest *PRI* vote in 1952 made the largest gains. Whereas in 1952 twelve states voted less than 75 percent for the *PRI,* in 1967 only two states were below this level. The fact that the lowest states increase most is of course partly a statistical phenomenon, since a state with a *PRI* percentage of 97 in 1952 cannot increase by much in 1967.

Third, correlational analysis is not very helpful in explaining the varied increases in states' *PRI* votes. Regardless of which of the possible dependent variables we use, the correlational analysis simply tells us that the more urbanized developed states increased their *PRI* percentage more than the rural states which were always high-*PRI*.

Can we reconcile the levels-of-urbanization hypothesis with the trend in *PRI* vote? We might argue as follows: if there are variables outside the model causing a net gain in *PRI* vote, those states urbanizing fastest should be gaining relatively less. But in analyzing urbanization rates and changes in *PRI* vote over time, I found no evidence that rapidly urbanizing states are becoming less strong for the *PRI*.

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[46] One of the interesting problems to be explored in the future concerns the interrelationships between turnout, opposition voting, and government outcomes from election to election. This problem has been treated in the United States by, among others, Walter Dean Burnham, "The Changing Shape of the American Political Universe," this Review, LIX (March, 1965), and Angus Campbell, "Surge and Decline: A Study of Electoral Change," *Public Opinion Quarterly*, 24 (1960).

[47] For example, the percentage gain from 1952 to 1967, the gain from 1955 to 1967, or the algebraic sum of changes in rank order between 1952 and 1967.

The second time-related problem concerns the causal effects of benefits on *PRI* vote rather than the cross-sectional association. I suggested earlier that benefits were going into low-*PRI* states. This is evident in the following table, which relates *PRI* vote in 1958 (beginning the period covered by benefits data) to the benefit level, 1959-63:[48]

TABLE 4: 1958 PRI VOTE IN RELATION TO BENEFITS

| | States above mean vote | States below mean vote |
|---|---|---|
| Ratio of investment to poverty characteristics in succeeding period | | |
| Above median | 7 | 9 |
| Below median | 12 | 4 |

Of the thirteen states whose *PRI* vote was below the national mean in 1958, nine received benefits in a ratio above the national median between 1958 and 1964. Table 5 shows the changes in *PRI* percentage in three election periods for these thirteen states. Between the 1958 and 1964 Presidential elections, the nine of these thirteen states receiving benefits above the national median *increased* their *PRI* vote 6.4 percent. The four states receiving benefits below the median *decreased* 2.5 percent. We can see that even among the below-mean *PRI* states in 1958, those receiving benefits in a high ratio to poverty levels were those who had done most poorly in *PRI* percentage change between 1955 and 1958. Although they made remarkable improvements between 1958 and 1964, in the most recent election no differences were evident.

TABLE 5: CHANGES IN PERCENTAGE PRI AMONG STATES WITH PRI VOTE BELOW MEAN IN 1958

| Benefits 1959–63 | 1955–58 | 1958–64 | 1964–67 |
|---|---|---|---|
| Ratio above median | −6% | +6.4% | −2% |
| Ratio below median | −4% | −2.5% | −2% |

The difficulty with this kind of analysis is obvious. We have little knowledge of the time lag involved in the benefits→votes relationship. The relationships shown here are based on very few cases. The Mexican data is suggestive of a linkage between investment and affective response, but it is definitely not conclusive.

The most significant of these time-dependent problems is the apparent paradox of a model predicting low *PRI* vote in the most modernized states, and the evidence that the greatest *PRI* gains are made in these same states. Such a phenomenon is not uncommon; for example, social mobilization and military intervention are negatively related in Latin America, but in the aggregate, both mobilization and military intervention are rising.

It is also true that variables whose effects cannot be measured here may have a substantial effect on the changes in *PRI* vote over time. One possible variable is migration: the movement of people to urban areas implies a shift of voters with a high likelihood of a *PRI* vote into low-*PRI* areas. Although urban experience might eventually decrease the *PRI* vote of immigrants, their aggregate vote is slightly more strongly *PRI* than that of long-term residents.[49]

Although it is generally beyond the scope of this paper to consider the reason for the *PRI*'s continuing dominance of elections, some speculation is appropriate. One important factor is the economic success of Mexico during this period. The *PRI* is a likely beneficiary in the short run from Mexico's prosperity. A second possibility is the skill of *PRI* leadership in responding to outcries from the citizenry. If one of the major sources of anti-*PRI* movements is the dislike of corrupt and inefficient officials rather than programmatic or ideological disagreements, then the *PRI* would be strengthened by replacing such officials and increasing its use of achievement criteria. Another significant variable may be the character of the opposition, expecially their feelings of efficacy in the electoral arena. The demoralizing effects of an inability to achieve even minimal electoral successes may strengthen the *PRI* even while other factors weaken its electoral position.

## VII. SUMMARY AND IMPLICATIONS

I presented a series of hypotheses concerning the bases of *PRI* support. The data indicate that about 75 percent of the variance in the *PRI* percentage in these six elections can be explained by four variables: level of urbanization, turnout, the structure of the opposition, and historical nonintegration. The original hypotheses now take on the following form:

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[48] The median benefit ratio was used because the Federal District (Mexico City) had a score almost five times as high as the next highest unit. Capital spending figures are being used instead of water programs because of correspondence between their time period and the elections.

[49] Cornelius, *op. cit.*, found that in 1958, 9 percent of urban-born people in urban areas voted for opposition parties, while only 3 percent of migrants into urban areas did so.

1. The higher the level of development (as measured by urbanization or other indicators) the less will be the *PRI* percentage. Turnout is unaffected by the level of development.
2. High rates of electoral participation are associated with high *PRI* percentage.
3. A condition of historical nonintegration is associated with lower levels of *PRI* vote and higher levels of turnout.
4. Turnout and vote are unaffected by presence on the US border.
5. The percentage of vote received by the *PRI* and the level of turnout are lower when there are more opposition parties.
6. The percentage of the population affiliated with the *PRI* does not affect either turnout or direction of vote.
7. Federal spending tends to be highest in low-*PRI* states and may increase *PRI* vote levels in the short-run.

To discuss the implications of these findings we must confront the fact that the *PRI* increased its support almost everywhere, while the conclusion flowing naturally from our model is that increasing development in Mexico should lessen dominance of the *PRI*. In fact, the most developed areas made the greatest gains between 1952 and 1967.

An easy way out of this dilemma would be the argument that the *PRI* may increase its support in the short run but in time will lose its predominant status. There is, however, no support for such a conclusion in these data. In fact, if Mexico continues to enjoy prosperity, and if the *PRI* continues to replace the worst of its officials, it may become even more powerful in the next few elections. Even the gradual shift from a subject-parochial political culture to a subject-participant one will not weaken the *PRI*, since the evidence presented here shows that a kind of "demonstration effect" operates to bring the *PRI* a higher vote when turnout and registration rise.[50] The dominance of the *PRI* may be threatened by economic recession or insufficient responsive capability, but these developments have certainly not yet occurred.

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[50] Almond and Verba, *op. cit.*, p. 17-26. A parochial political culture implies a low frequency of orientations toward the political system in such areas as inputs, outputs, participation, the "system" as a general object, etc.: "In a subject political culture, there is a high frequency of orientations toward a differentiated political system and toward the output aspects of the system, but orientations toward specifically input objects, and toward the self as an active participant, approach zero . . . In a participant culture, members are oriented to both the input and output sides of the system."

# THE ELECTORAL SYSTEM OF CANADA*

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The working of the electoral system in Canada is investigated in this paper. The object is to identify the more important factors which go to determine the share of seats in the federal House of Commons won by a political party at a general election. Factors considered are share of vote, distribution of the vote and number of candidates in the field. The responsiveness of share of seats to variations in these factors is estimated by fitting linear equations by least squares to data for the fourteen federal general elections which took place in Canada between 1921 and 1965.

## I. THE REPRESENTATION FUNCTION

The single-member-constituency system of election has cast up some rather puzzling results in Canada. Exhibited in Table 1 are shares of seats and shares of vote won by the parties contesting the general elections of 1930 and 1935. Two things stand out from the results. The first is that the electoral system apportions seats among the parties in a far from even-handed way: the party gaining the largest proportion of the vote is generously rewarded with seats at the expense of the other parties. This tendency, it should be said, is not peculiar to Canada but is observed wherever the single-member-constituency system is used.[1] Second, the system seems to suffer from a lack of consistency. Compare the fortunes of the Liberal party in the two elections: in 1935, with a slightly reduced vote, the party won close to double the share of seats it held in 1930. Again, compare the performance of the Progressive party in 1930 with that of the Reconstruction party in 1935. There is little evidence here of a simple relationship between a party's share of seats and its share of vote.

TABLE 1. RESULTS OF GENERAL ELECTIONS IN CANADA, 1930 AND 1935

| Party | Election of 1930 |  | Election of 1935 |  |
|---|---|---|---|---|
|  | Seats (%) | Vote (%) | Seats (%) | Vote (%) |
| Liberal | 37.1 | 45.5 | 70.6 | 44.9 |
| Conservative | 55.9 | 48.7 | 16.3 | 29.6 |
| Progressive | 4.9 | 3.2 | — | — |
| Co-operative Commonwealth Federation | — | — | 2.9 | 8.9 |
| Social Credit | — | — | 6.9 | 4.1 |
| Reconstruction | — | — | 0.4 | 8.7 |
| Other | 2.0 | 2.7 | 2.9 | 3.7 |

Source: Howard A. Scarrow, *Canada Votes*, (New Orleans, 1962), Tables 24 and 28.

Now there is nothing in the rules of the single-member-constituency system of election to ensure that a party's share of seats will be related in a predictable way to its share of vote. In a two-party system, a party winning 40 per cent of the vote might win anything from nil to close to 80 per cent of the seats, depending on how the vote is distributed among the constituencies; with more than two parties, the range of possible outcomes is even greater. In fact, however, share of seats does tend to be closely dependent on share of vote in single-member-constituency systems—at least outside Canada. Dahl has fitted linear equations to time-series data for Congressional elections in the United States and found that more than nine-tenths of the variance of share of seats was capable of explanation by the behavior of share of vote.[2] In Great Britain, the so-called "cube law," which states that the proportion which party *A*'s share of seats bears to party *B*'s will vary as the cube of the proportion of *A*'s share of vote to *B*'s, has yielded quite accurate predictions of election outcomes. The implied relationship between share of seats and share of vote is non-linear. The rationale of the cube law has been investigated by the British statisticians Kendall and Stuart, who discovered that the construct rested on certain conditions governing the distribution of the proportions of vote won by a party in the

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* I wish to thank John C. Courtney and Peter C. Dooley for helpful criticism. Some preliminary results of this study were reported in a paper read at the annual meeting of the Canadian Political Science Association in 1967.

[1] See Maurice Duverger, *Political Parties: Their Organization and Activity in the Modern State* (London, 1954), esp. pp. 373–4; and Douglas W. Rae, *The Political Consequences of Electoral Laws* (New Haven, 1967).

[2] Robert A. Dahl, *A Preface to Democratic Theory* (Chicago, 1956), pp. 148–9. Dahl regresses Democratic share of *major-party* seats on that party's share of *major-party* vote.

constituencies.[3] The conditions were found to be approximately satisfied in Great Britain.

In Canada, neither the linear nor the cube-law hypothesis offers a convincing account of election outcomes. For the major (Liberal and Conservative) parties over the period from 1921 to 1965, no more than about three-quarters of the variance of share of seats can be explained by changes in share of vote, assuming linearity. The cube law, in its original (two-party) form, performs no better.[4] Considering that Canada is a multi-party system, the inapplicability of the cube law is not surprising. The mechanics of the cube law require, among other things, that the proportion of the vote needed to win a contest be one-half. In a three- or four-party system, a contest can be won with a good deal less than one-half of the vote, and the required proportion generally cannot be specified *a priori*.

There are, of course, many other forms of the relationship between a party's share of vote and its share of seats which might be canvassed. It seems unlikely, however, that the problem of explaining election outcomes in Canada is wholly or even largely one of discovering the correct functional form of a two-variable relationship. What does seem likely is that there are factors other than share of vote which play a part in the determination of election outcomes. To elicit the influence of these factors a multivariate analysis must be undertaken.

We begin by positing the dependence of the $i$th party's share of seats $S_i$ on its share of vote $V_i$, $K$ variables $X_1, \cdots, X_k$, and a variable $u$:

$$S_i = f(V_i; X_1, \cdots, X_k; u)$$

It is convenient to have a name for this relation, and "representation function" seems apt. The variables $X_1, \cdots, X_k$ are as yet unspecified factors which, along with $V_i$, exert systematic influences on $S_i$. The remaining variable, $u$, is an error term whose inclusion acknowledges the possibility of inexactness arising from incomplete specification, data errors and random disturbances. The expected value of $u$ is assumed to be zero.

Two kinds of decisions must be made in the course of specifying the representation function. First, the form of the dependence of $S_i$ on the independent variables has to be decided upon. We adopt the assumption of linear form, principally because of its simplicity. (Something more will be said about this assumption later on.) Second, the variables $X_1, \cdots, X_k$ must be specified. Variables considered are numbers of candidates placed in the field by the several parties and an aspect of the distribution of the parties' vote among the constituencies. We discuss now the ways in which these variables may be supposed to figure in the representation function.

*Number of candidates.* The number of candidates presenting themselves for election has varied a great deal in recent Canadian experience. The major parties usually have contested all or almost all constituencies, though in several elections they named no candidates in many. The number of other ("minor-party") candidates has varied from fewer than one hundred to more than five hundred.

The proportion of vote necessary for victory in a constituency can be taken to be dependent in a loose way on the number of candidates in contention. For two candidates, something more than one-half of the vote is required; for three, it might be little more than one-third (though it might be a great deal more); for four, perhaps little more than one-quarter; and so on. A party winning 40 per cent of the national vote might win relatively few seats in a two-party contest. In an election involving four parties, 40 per cent of the national vote might be enough to win a majority of seats.

It is worthwhile to inquire more closely into the way in which variations in the number of candidates affect election outcomes. Suppose there are three parties, $A$, $B$, and $C$; and suppose further, for the sake of tidiness, that the voting population is fixed from election to election and everyone turns out. Each of the three parties enters a candidate in all $n$ constituencies in election I. In a constituency $J$, $C$'s candidate finished a poor third in election I, and $C$ does not name a candidate in $J$ in election II. Imagine now that in the other $n$-1 constituencies the results of election II are identical in every respect with those of election I. The only changes in the system between elections I and II, then, are those following from $C$'s vacating $J$.

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[3] M. G. Kendall and A. Stuart, "The Law of the Cubic Proportion in Election Results," *British Journal of Sociology*, Vol. 1 (1950), 183–96.

[4] T. H. Qualter, in a recent study, puts forward a "modified cube law" for the multi-party case and applies it to Canada. Because of differences in intent and method it is difficult to compare Qualter's results with those presented here. See T. H. Qualter, "Seats and Votes: An Application of the Cube Law to the Canadian Electoral System," *Canadian Journal of Political Science*, Vol. 1 (1968), 336–344. It may be noted that both the two-party cube law and Qualter's "modified cube law" are special cases of the "weak proportionality" function proposed by Theil. See Henri Theil, "The Desired Political Entropy," this REVIEW, Vol. LXIII (June, 1969), 521–525.

By abandoning a (by hypothesis) hopeless cause in $J$, party $C$ will have augmented the "efficiency" of its vote in the following sense: the party wins the same number of seats as before, but with a smaller share of the total vote. We may be permitted to infer from this that, had it won as large a share of vote in II as in I, $C$ could have won more seats in II.[5] That is, for a given share of vote, a party is predicted to win more seats when the number of constituencies it enters is reduced. There is a good deal of common sense in this prediction, which simply says that a given vote will yield more seats for a party when it is spread over fewer candidates.

Can it be taken for granted that the same reasoning applies when a party *increases* the number of constituencies it enters? There is a certain asymmetry here. It could be supposed that a party would abandon only palpably hopeless causes. One cannot suppose, however, that every new candidature is of the hopeless variety: the facts state otherwise. It remains, however, that the probability of a newcomer's winning a seat is quite small. Further, against the effects of a victory on a party's efficiency might have to be set the effects of perhaps a dozen defeats. The asymmetry must be allowed to stand, but it does not necessarily force a revision of the hypothesis that a party's efficiency will vary inversely with the number of its candidates.

We revert now to the simple model to examine the consequences for $A$ and $B$ of $C$'s decision not to contest $J$. Now it is clear that, because $C$ wins the same number of seats with a smaller share of the vote in election II, $A$ and $B$ *taken together* must win the same number of seats with a larger share of the vote. At least one and possibly both of $A$ and $B$ will suffer a worsening in efficiency.

There is one case in which it can be predicted confidently that the efficiency of both $A$ and $B$ will be reduced. This case involves the following assumptions: (a) that, as a result of $C$'s vacating $J$, the number of votes going to each of $A$ and $B$ in $J$ is larger; (b) that $C$'s stepping down does not cause the decision in $J$ to be different. Assumption (a) seems fairly plausible even though arguable. It amounts to the supposition that, in election II, some of $C$'s adherents from election I days will go over to $A$ and some will go over to $B$. Assumption (b) has somewhat less to recommend it. The former adherents of $C$ might divide between $A$ and $B$ in such a way as to alter the decision. Suppose that $A$ won $J$ in election I but that in election II, with $C$'s candidate absent, $B$ emerged the victor. The efficiency of $A$'s vote clearly will have deteriorated: the party wins a larger vote and fewer seats than before. But the effect on the efficiency of $B$, which wins a larger vote as well as more seats, is problematical. Since the assumptions represent sufficient but not necessary conditions, a worsening of $B$'s efficiency cannot be ruled out. Neither, however, can it be predicted with assurance.

In summary, our expectations take the following form. When a party reduces the number of its candidates, other things being equal, its efficiency will be improved; that is, for a given share of vote, the party will win more seats. The efficiency of at least one of its rivals must be reduced and that of all rivals might be. Effects of opposite kind can be anticipated throughout when a party increases the number of constituencies entered.

In the empirical work to follow, the number of candidates placed in the field by the $i$th party is denoted by $N_i$. For ease of interpretation of the coefficients $N_i$ is expressed on the basis of an electoral system comprising 265 constituencies. (The actual number of seats varied over the 1921–65 period from 235 to 265.)

*Distribution of the Vote.* The share of seats won by a party will depend critically on the way in which its vote is distributed among the constituencies. The handling of distributional factors in an aggregative analysis is, it must be conceded, an awkward matter. Further, where more than two parties contend, as in Canada, such factors can be highly complex. There is, however, at least one aspect of the distribution of the vote which can be incorporated into the present analysis.

In Canada, at least until the most recent (1966) redistribution, substantial variation in the populations of constituencies has been permitted. Constituency populations have tended, moreover, to vary along a dimension which is relevant to the determination of voters' party preferences. Rural constituencies generally have contained fewer voters than urban constituencies. Because a rural seat contains fewer voters, it can be won with a smaller proportion of the *national* vote than is required to win an urban seat, other things being equal. Thus, the share of vote required to win a given share of seats might be relatively small if the vote is concentrated in small rural constituencies. It will be convenient to state the hypothesis in an alternative form: a party will win more (fewer) seats for a given share of vote, other things being equal, the greater its relative strength in small (large) constituencies.

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[5] The inference would be fully justified if the representation function were continuous and monotonic increasing. Obviously this requirement is not met in fact: the addition of a handful of votes to a party's total will not always result in additional seats won. It is sufficient for present purposes to assume that the probability of a party's winning an additional seat with a modest augmentation of its vote is not zero.

To test this hypothesis it is necessary to devise an index representing the relative strength of a party in size-classes of constituencies. An index, denoted by $B_i$, was constructed in the following way. Constituencies larger than mean size (according to the number of votes cast) were identified, and the aggregate share of vote cast in favor of the $i$th party in those constituencies calculated. This large-constituency share was then divided by $V_i$, the share of the vote received by the party over all constituencies, and the resulting quotient was multiplied by 100. An index of $B_i = 110$, for instance, would connote that the $i$th party's performance in large constituencies was 10 per cent better, in relative terms, than its performance in all constituencies (e.g., the party might have won 55 per cent of the aggregate vote in large constituencies and 50 per cent of the aggregate vote nationally). A value of 100 would indicate that the party did equally well in large constituencies and all constituencies. The expectation is that the share of seats won by the $i$th party will vary inversely with $B_i$, other factors held constant.

## II. EMPIRICAL ESTIMATES

We turn now to the task of estimating representation functions embodying the hypotheses set out. The representation functions are estimated by fitting linear regression equations to observed values of the relevant variables. The data pertain to the fourteen elections which took place in Canada from 1921 to 1965 and are displayed in an Appendix. Candidates are divided exhaustively into three groups: Conservative, Liberal and "minor-party," the latter group being a conglomerate of all candidates who are neither Conservative nor Liberal. Symbols used to represent the variables and to identify political parties are listed below:

$S_i$: Share of seats (per cent) won by the $i$th party;
$V_i$: Share of vote (per cent) won by the $i$th party;
$N_i$: Number of candidates (basis 265 constituencies) placed in the field by the $i$th party;
$B_i$: Index of relative performance of the $i$th party in constituencies larger than mean size;
$i = C$: Subscript identifying Conservative party;
$i = L$: Subscript identifying Liberal party;
$i = M$: Subscript identifying minor parties.

*Regression equations.* For the Conservative party, $S_C$ initially was regressed on the variables $V_C$, $N_C$, $B_C$, $N_L$, and $N_M$. It was found that the coefficient of $N_L$ was effectively zero; seemingly, conservative share of seats has not been sensitive to changes in the number of Liberal candidates. The variable was dropped and the equation fitted again, with the outcome:[6]

$$(1) \quad S_C = \underset{(0.198)}{2.897^{**}V_C} - \underset{(0.099)}{0.241^{**}N_C} - \underset{(0.277)}{0.819^{*}B_C} + \underset{(0.0098)}{0.0550^{**}N_M} + 53.49$$

$$\bar{R}^2 = .964, \qquad \text{S.E.E.} = 3.39$$

The results are, on the whole, quite gratifying. Fit is good, all coefficients can be supposed with some confidence to differ from zero, and signs capable of prior prediction are as expected. The Conservative party's share of seats is shown to vary directly with its own share of vote and with the number of minor-party candidates, and inversely with the number of its own candidates and with its relative popularity in larger-than-average constituencies, other things held constant in each case. The standard error of estimate at 3.39 (per cent of total seats) amounts to about nine seats in a 265-seat House of Commons.

A trial fitting of the representation function for the Liberal party turned up one variable ($B_L$) whose coefficient fell somewhat short of conventional significance levels and another ($N_C$) whose coefficient was patently insignificant. The latter only was discarded, and the equation became:

$$(2) \quad S_L = \underset{(0.303)}{3.217^{**}V_L} - \underset{(0.146)}{0.356^{*}N_L} - \underset{(0.598)}{0.821B_L} + \underset{(0.0129)}{0.0656^{**}N_M} + 62.85$$

$$\bar{R}^2 = .902, \qquad \text{S.E.E.} = 4.97$$

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[6] Shown in parentheses below the sample coefficients are the corresponding standard errors; $\bar{R}^2$ is the coefficient of determination corrected for degrees of freedom; S.E.E. is the standard error of estimate. Asterisks raised above the coefficients indicate significance level according to a two-tailed $t$-test, as follows: **significant at the .01 level; *significant at the .05 level. A useful and relatively non-technical introduction to regression analysis is Mordecai Ezekiel and Karl A. Fox, *Methods of Correlation and Regression Analysis* (New York, 3rd ed., 1959).

Again, signs are as expected, although the coefficient of $B_L$ is not significant. With $\bar{R}^2$ of .902 the fit of the equation is somewhat less close than that of the Conservative equation.

A rather different specification was adopted for the minor-party equation. It did not seem appropriate in the case of such a conglomerate to use an index of relative strength in constituencies of different sizes. Also, when $V_M$, $N_M$, $N_C$, and $N_L$ were entered together as explanatory variables, a severe problem of collinearity arose. Minor-party share of vote is highly dependent on the number of candidates placed in the field by the several parties, and the consequent redundancy of information when the variables are entered in each other's presence makes it difficult to obtain reliable estimates of the coefficients.[7] If an estimate was to be made at all, it seemed necessary to jettison one or more of the variables. Unfortunately, all would appear to belong in the representation function, and dropping variables might be merely to exchange one source of unreliability for another. We might suppose, however, that, whatever other variables are relevant, $V_M$ and $N_M$ ought to figure in the equation.

$$(3) \qquad S_M = \underset{(0.233)}{1.237^{**}V_M} - \underset{(0.0117)}{0.0360^{**}N_M} + 0.99$$

$$\bar{R}^2 = .718, \qquad \text{S.E.E.} = 3.47$$

Both coefficients are significant and the $\bar{R}^2$ is of a respectable order. However, the arbitrariness of this specification becomes evident when $N_M$ is replaced by $N_L$. The fit of this equation (not shown) is as close as that of (3), and the coefficient of $N_L$ turns out to be highly significant and *negative*. Replacing $N_L$ in turn by $N_C$, the latter's coefficient also bears a negative sign though it is not significant. The negative signs persist through all configurations of variables. There are indications here that when a major party withdraws from contests the efficiency of the minor-party vote is *improved*, which in turn suggests that commonly such contests are won by minor-party candidates.[8] The tactics of multi-party politics no doubt figure in this phenomenon. A major party might withdraw from a contest with the hope of delivering the constituency into minor-party hands and thwarting the ambitions of the other major party; or, recognizing that a minor-party candidate will win in any case, the major party might withdraw as a gesture of appeasement. The matter is, perhaps, of more historical than current interest, since in the last half-dozen elections the major parties have fielded complete slates of candidates with only a few exceptions.

The empirical estimates establish the importance of several factors in the working of the electoral system in Canada. In the case of the Conservative party, as many as four variables are shown to contribute significantly to the explanation of share of seats won; in the case of the Liberals, three. As noted, share of vote alone is able to account for only about three-quarters of the variance of share of seats won by a major party. The addition of numbers-of-candidates variables and an index representing one aspect of the distribution of the vote raises the proportion of variance explained to nine-tenths or higher. For the minor parties, the relevance of at least one variable besides share of vote is suggested, though because of collinearity the precise identity of the other variable or variables remains in doubt.

*Some features of the system.* From the coefficients estimated in equations (1)–(3) can be elicited a good deal of information concerning the working of the electoral system in Canada. We confine attention here to a few of the more important and interesting results.

The relationship which perhaps is of most interest is that between a party's share of seats and its share of vote when other factors are held constant. For the major parties, according to the coefficients of $V_C$ and $V_L$ in (1) and (2), a change in share of vote of one percentage point is transformed into a change in share of seats of about three percentage points in the same direction. Thus, in a House of Commons of 265 seats, an additional percentage point of vote won by a party would gain about eight seats. It is interesting that the estimated "incremental transformation rate" of 3.0 or thereabouts is quite close to the rate yielded by the cube law in Great Britain and to that estimated for Congressional elections in the United States.[9] That three systems should exhibit a regularity of this kind is indeed remarkable in view of the lack of formal restrictions on the transformation rate under the single-member-constituency method of election.

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[7] For a succinct discussion of this problem consult H. M. Blalock, Jr., "Correlated Independent Variables: the Problem of Multicollinearity", *Social Forces*, Vol. 42 (1963), 233–237.

[8] It seems clear that the minor parties have, on the whole, performed better when they were unopposed by one of the major parties, though the results are mixed. In the election of 1921, for example, the Liberal party did not name candidates in 33 constituencies, and in 28 of them minor-party candidates were successful; the Conservative party passed up 27 seats, of which only five were won by minor-party candidates.

[9] The cube law implies a non-linear relationship between share of seats and share of vote, i.e., the incremental transformation rate is not a con-

The transformation rate for the minor parties is substantially lower than the major-party rate. In fact, the coefficient of $V_M$ in (3) is less than half those of $V_C$ in (1) and $V_L$ in (2). The minor parties are at a relative disadvantage, the most plausible explanation of which is the generally lower level of the minor-party vote.

This, however, raises a question concerning the form of the relationship between share of seats and share of vote. If, in general, the transformation rate varies with share of vote, the linearity assumption relied on thus far might not be suitable. Several rough-and-ready tests for non-linearity were carried out, and in none was its presence indicated. On the other hand, experimentation with disaggregated data, in which share of vote varied over a wide range, did turn up strong evidence of non-linearity. The following tentative explanation is offered: the underlying relationship between a party's share of seats and its share of vote is in fact non-linear; but over the limited range in which share of vote is observed in the present samples, the relationship can be closely approximated by a straight line.

When the number of minor-party candidates is increased, the share of seats won by a major party for a given own share of vote is augmented. According to equation (1), the increase in Conservative share of seats when an additional minor-party candidate enters the field is .055 per cent (of the seats in the House); equation (2) offers an estimate of .066 per cent in respect of the Liberals. There is no necessity for the two coefficients to be equal: changes in minor-party activity could have quite different effects on the two major parties. However, the two sample coefficients here are within one standard error of each other, and the inference of no difference would seem to be called for.

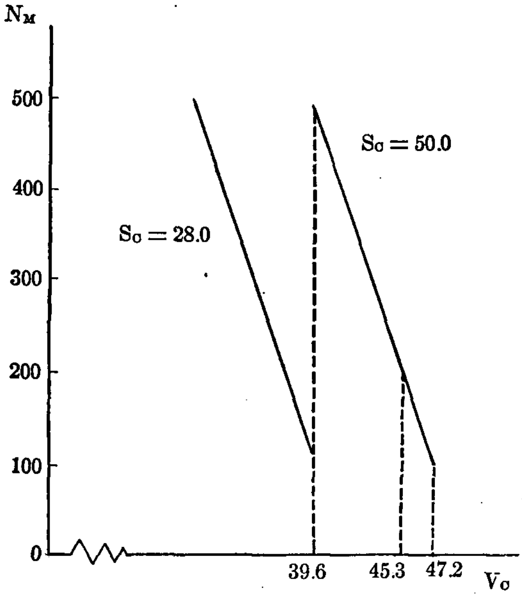

FIGURE 1

The dependence of major-party share of seats on the number of minor-party candidates explains the ability of parties to win majorities in the House of Commons with much less than a majority of the vote cast in their favor. Figure 1 provides an illustration, using the coeffiicients for the Conservative party estimated in equation (1). Measured along the horizontal axis is $V_C$, and along the vertical axis $N_M$. The slanted lines ("equal-representation lines") trace out combinations of $V_C$ and $N_M$ which yield stated levels of $S_C$.[10] (It is assumed that the Conservatives contest all seats and perform equally well in large and small constituencies, i.e. $N_C = 265$ and $B_C = 100$). With 100 minor-party candidates in the field the Conservatives are estimated to require 47.2 per cent of the vote to win 50 per cent of the seats. Setting $N_M$ at 200, some 45.3 per cent of the vote is needed by the major party to win 50 per cent of the seats; at 500, only 39.6 per cent. In contrast, 39.6 per cent of the vote is indicated to yield only about 28 per cent of the seats when there are as few as 100 minor-party candidates in the field.

Since the independent variables in an equation are related to a common variable $S_i$, they can be brought into relation with each other to answer questions of this sort: By what amount must one independent variable be changed to compensate for the effect on $S_i$ of a given change in another independent variable?

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stant but rather varies with share of vote. However, James G. March reports that for shares of vote ranging from 40 per cent to 60 per cent the cube law can be approximated closely by a linear equation which fixes the transformation rate at about 2.8. See James G. March, "Party Legislative Representation as a Function of Election Results," *The Public Opinion Quarterly*, Vol. XXI (1957/58), 521–542. Dahl (*op. cit.*, pp. 148–149) elicited transformation rates for the Democratic party of 2.50 in House of Representatives elections and 3.02 in Senate elections.

[10] The slanted (equal-representation) lines are truncated at $N_M = 100$ and $N_M = 500$ to remain well within the range of observed values of the variable in the sample.

TABLE 2. STANDARD REGRESSION COEFFICIENTS (BETA WEIGHTS)

| Equation | $V_C$ | $V_L$ | $V_M$ | $N_C$ | $N_L$ | $N_M$ | $B_C$ | $B_L$ |
|---|---|---|---|---|---|---|---|---|
| (1) | 1.37 | | | −0.18 | | 0.47 | −0.22 | |
| (2) | | 1.01 | | | −0.31 | 0.64 | | −0.13 |
| (3) | | | 1.47 | | | −0.85 | | |

Source: Calculated from coefficients of Equations (1)–(3) and standard deviations of variables.

Of special interest is the way in which variations in the "other" factors compensate for gains and losses in a party's voting support.

In this connection, the slope of the equal-representation lines in Figure 1 can be given a useful interpretation. The slope indicates the rate at which $V_C$ must be substituted for $N_M$ to maintain constant $S_C$. Holding $N_C$ and $B_C$ constant, we can write, from equation (1),

$$\Delta S_C = 2.897\Delta V_C + 0.055\Delta N_M$$

where Δ stands for "change in." Along a given equal-representation line, $\Delta S_C = 0$; hence,

$$0 = 2.897\Delta V_C + 0.055\Delta N_M,$$

and the slope of the equal-representation line is

$$\frac{\Delta N_M}{\Delta V_C} = -\frac{2.897}{0.055}$$
$$= -52.7$$

Thus, an increase of about 53 in the number of minor-party candidates will have an effect on Conservative share of seats which just compensates for the effect of a decrease in its (the Conservative party's) share of vote of one percentage point. Put otherwise, the Conservatives can give up one per cent of the vote for every 53 additional minor-party candidates in the field and maintain a given share of seats, other factors held constant.

Substitution rates can be calculated in a similar way for $V_C$ in substitution with the other independent variables. Taking $B_C$,

$$\frac{\Delta B_C}{\Delta V_C} = 3.5$$

If there were a shift in the party's base of support towards small constituencies such that $B_C$ fell by 3.5 (e.g., from 100 to 96.5) the party could win a given share of seats with a share of vote one percentage point smaller. Finally, for $N_C$,

$$\frac{\Delta N_C}{\Delta V_C} = 12.0$$

Withdrawing 12 Conservative candidates from the field has the same effect on $S_C$ for given $V_C$ as an increase in $V_C$ of one percentage point. Substitution rates for the Liberal party, calculated from the coefficients in equation (2), are fairly similar throughout.

It would be useful, finally, to know something about the relative importance of the several independent variables in producing changes in election outcomes. As they stand, the regression coefficients do not offer a reliable basis for judging the relative importance of the variables: the size of a coefficient will reflect the (often arbitrarily-selected) units in which the independent variable is measured. For comparison to be meaningful, some standardization procedure is needed. Here we may make use of standard regression coefficients, often called Beta-weights, which show the change in the dependent variable, in own-standard deviation units, when an independent variable is changed by one own-standard deviation.[11] Beta-weights for the variables in equations (1)–(3) are shown in Table 2.

On the basis of the absolute values of the Beta-weights, own share of vote is the independent variable to which share of seats is most responsive in all three cases: a not unexpected result. Ranking next in importance throughout is number of minor-party candidates. We may conjecture that the seemingly eccentric changes in party standings which occur from time to time in Canadian elections are due largely to changes in the number of minor-party candidates. Reverting to the data of Table 1, it is doubtless significant that fewer than 100 minor-party candidates contested the election of 1930 while more than 400 contested that of 1935. The very substantial increase in minor-party participation in 1935 led to a correspondingly substantial increase in the efficiency of the Liberal vote.

*Qualifications and a test.* The empirical estimates are subject to several qualifications which ought to be recorded. First, certain specification problems which affect the interpretation of the coefficients are left unresolved. In the case of the minor parties, several formulations produced equally good fits; the major-party equations, fortunately, yielded less ambiguous results. The difficulties with the minor-party equation may be in part the result of lumping together all "other" candidates. Disaggregation would be desirable, but it would mean reducing the size of an already very small sample (no one third party's life spans the sample period). Second, it is possible that the estimated coefficients are biased because of the omission of relevant variables. The only way to guard against such biases is to bring all likely variables within the compass of the analysis. A few other variables were tried, but none appeared to add anything. It may be worth reporting that attempts to uncover possible effects of the periodic re-drawing of constituency boundaries (the effects assumed to be in favor of the party in power) turned up nothing. The results may reflect the method, which was distinctly unrefined. Third, a doubt concerning one of the variables must be confessed to. The index $B_i$ is constructed from data which are subject to the influence of other explanatory variables, and its independence is open to question. The index is moderately correlated in the sample with several other variables (see correlation matrix, Appendix).

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[11] See H. M. Blalock, Jr., *Social Statistics* (New York, 1960), p. 345.

The general election which took place in Canada in 1968 offers a limited but valuable opportunity to test the capabilities of the equations. The relevant data (recorded in the Appendix) were inserted into equations (1)–(3) to produce the estimates shown in Table 3. For the Liberals and the minor parties, the equations perform very well; the estimate for the Conservatives is not so close to the mark. The results are as good as or better than one might have expected on the basis of the standard errors of estimate in the sample period.

TABLE 3. ESTIMATED AND ACTUAL SHARES OF SEATS, ELECTION OF 1968

| Party | Estimated Share of Seats (%) | Actual Share of Seats (%) | Error (%) |
|---|---|---|---|
| Conservative | 31.5 | 27.3 | +4.2 |
| Liberal | 57.3 | 58.7 | −1.4 |
| Minor Parties | 13.6 | 14.0 | −0.4 |

Source: Estimated share of seats obtained by inserting values of independent variables (see Appendix) into equations (1), (2) and (3).

## III. CONCLUSIONS

The most general conclusion to be drawn from the analysis is that the determination of election outcomes in Canada is a complex process in which the influences of several factors combine. By the use of regression analysis it has been possible to tentatively identify several—hopefully, the most important—variables which figure in the process. For the major parties, the variables shown to be relevant are own share of vote, number of own candidates, number of minor-party candidates and (in one case) relative strength of the major party in size-classes of constituencies. Certain problems stood in the way of a satisfactory account of minor-party share of seats, but the importance of at least one variable besides share of vote is indicated.

For the specifications adopted, the variable ranking next in importance to own share of vote is number of minor-party candidates. The greater the number of minor-party candidates in the field, the smaller the share of vote needed by a major party to attain a given share of seats, other things remaining constant. The regression coefficients provide a quantitative measure of the effect on the efficiency of the major-party vote of changes in the level of minor-party activity.

It is estimated that, for the major parties, an increase in share of vote of one percentage point will bring about an increase in share of seats of about three percentage points. This is similar to rates calculated for the two-party systems of Great Britain and the United States, but much larger than the rate estimated here for the minor parties in Canada.

In respect of method, the study offers some evidence of the potentialities of multiple regression analysis in the investigation of electoral systems. There can be few electoral systems so innocent of complexity that their working can be adequately described in a relationship between two variables, share of seats and share of vote. Other factors can—in Canada, at least, do—affect election outcomes.

## APPENDIX

### DATA AND SOURCES

Data:

| Election | $S_L$ | $S_C$ | $S_M$ | $V_L$ | $V_C$ | $V_M$ | $N_L$ | $N_C$ | $N_M$ | $B_L$ | $B_C$ |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 1921 | 49.4 | 21.3 | 29.4 | 40.7 | 30.3 | 29.0 | 228 | 235 | 254 | 96 | 97 |
| 1925 | 40.4 | 47.3 | 12.2 | 39.8 | 46.5 | 13.8 | 232 | 253 | 141 | 96 | 110 |
| 1926 | 52.2 | 37.1 | 10.6 | 46.1 | 45.3 | 8.7 | 238 | 252 | 83 | 98 | 106 |
| 1930 | 37.1 | 55.9 | 6.9 | 45.5 | 48.7 | 5.9 | 253 | 252 | 85 | 101 | 101 |
| 1935 | 70.6 | 16.3 | 13.1 | 44.9 | 29.6 | 25.4 | 263 | 251 | 450 | 97 | 100 |
| 1940 | 73.9 | 16.3 | 9.8 | 51.5 | 30.7 | 17.9 | 263 | 230 | 228 | 98 | 101 |
| 1945 | 51.0 | 27.3 | 21.6 | 41.1 | 27.4 | 31.5 | 262 | 221 | 545 | 100 | 101 |
| 1949 | 73.7 | 15.6 | 10.7 | 49.5 | 29.7 | 20.8 | 263 | 252 | 342 | 98 | 102 |
| 1953 | 64.5 | 19.2 | 16.3 | 48.9 | 31.0 | 20.1 | 265 | 248 | 384 | 95 | 102 |
| 1957 | 39.6 | 42.3 | 18.1 | 40.9 | 38.9 | 20.1 | 265 | 257 | 339 | 97 | 104 |
| 1958 | 18.5 | 78.5 | 3.0 | 33.6 | 53.6 | 12.8 | 265 | 265 | 301 | 97 | 101 |
| 1962 | 37.7 | 43.8 | 18.5 | 37.2 | 37.3 | 25.6 | 264 | 265 | 486 | 101 | 95 |
| 1963 | 48.7 | 35.8 | 15.5 | 41.7 | 32.8 | 25.4 | 265 | 265 | 492 | 104 | 93 |
| 1965 | 49.4 | 36.6 | 14.0 | 40.2 | 32.4 | 27.4 | 265 | 265 | 481 | 101 | 93 |
| 1968* | 58.7 | 27.3 | 14.0 | 45.5 | 31.4 | 23.1 | 264 | 264 | 443 | 106 | 90 |

Sources: $S_i$, $V_i$, 1921–1958: H. A. Scarrow, *Canada Votes* (New Orleans, 1962); 1962–1968: *Report of the Chief Electoral Officer* (Ottawa) 25th–28th general elections; $N_i$, $B_i$ computed from data in above sources, as follows:

$$N_i = \frac{265 n_i}{n}$$

where $n_i$ is the number of candidates entered by the $i$th party and $n$ is the total number of seats in the (forthcoming) House of Commons;

$$B_i = 100 \left( \frac{\Sigma v_{ij}}{\Sigma v_j} \div V_i \right), \qquad v_j > \bar{v},$$

where $v_{ij}$ is the number of votes won by the $i$th party in the $j$th constituency and $\bar{v}$ the mean number of votes cast in the $n$ constituencies. Where not all constituencies were entered by the $i$th party, data for uncontested constituencies were deleted in the computation of the index.

Data used differ slightly in some instances from those contained in the recent compilation by J. M. Beck in *Pendulum of Power* (Scarborough, 1968), which came to my attention too late to permit use here. The differences are small, and cannot be supposed to affect the argument.

* Not used in the calculation of regression coefficients.

### CORRELATION BETWEEN EXPLANATORY VARIABLES

| | $V_L$ | $V_C$ | $V_M$ | $N_L$ | $N_C$ | $N_M$ | $B_L$ | $B_C$ |
|---|---|---|---|---|---|---|---|---|
| $V_L$ | 1 | −.43 | −.17 | .06 | −.43 | −.25 | −.16 | .22 |
| $V_C$ | | 1 | −.81 | −.27 | .47 | −.63 | −.07 | .42 |
| $V_M$ | | | 1 | .25 | −.24 | .85 | .18 | −.60 |
| $N_L$ | | | | 1 | .24 | .66 | .35 | −.42 |
| $N_C$ | | | | | 1 | .08 | .29 | −.24 |
| $N_M$ | | | | | | 1 | .37 | −.64 |
| $B_L$ | | | | | | | 1 | −.65 |
| $B_C$ | | | | | | | | 1 |

# A NOTE ON OVERSEAS CHINESE POLITICAL PARTICIPATION IN URBAN MALAYA*

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*University of Rochester*

## I. INTRODUCTION

The object of this note is to demonstrate that generalizations about political participation may be invalid when applied to "developing" or "transitional" societies. Specifically, the relationship between rates of voter turnout and levels of education for urban Chinese in Malaya is not consistent with results reported for Western societies.

A geographical classification of bibliographic entries in Lester Milbrath's *Political Participation*[1] discloses a very interesting statistic: only 3 of the 288 listed entries concern the transitional societies of Asia, Africa and Latin America. The lack of data on developing areas may, in large measure, explain the emphasis placed on studies of political participation in North America and Western Europe. Although there is more research on transitional societies today, most studies still focus on advanced industrial societies. The validity of the generalizations presented in *Political Participation,* therefore, is restricted to North America and Western Europe.

Using data collected in Malaya (1957), I examine four of Milbrath's hypotheses. These include:

(1) higher education increases participation (p. 122);
(2) middle-aged persons participate more than young or old persons (p. 134);
(3) men are more likely to participate than women (p. 133); and
(4) religion affects participation (p. 137).

## II. METHOD

The dependent variable in each of these hypothesized relationships is participation, measured by percentage voter turnout among the population. With the exception of religion, measurement procedures need little discussion. Sex, age and education are evident. The different religions in Malaya are Buddhism, Taoism, Islam, Hinduism, and Christianity.

The data which I present were collected by Survey Research Malaysia during February-March 1967 in Kuala Lumpur and Penang, Malaya. A two-stage sample survey was designed to select 500 households in which adults could be enumerated and selected for interview against a list of qualifying numbers. The field work produced 374 successful adult interviews in Penang and 359 in Kuala Lumpur, the two largest and commercially most important cities in Malaya. The results describe only reported Chinese voting behavior at the 1964 Parliamentary or State Legislative Assembly elections (the most recent general elections preceding the date of the survey); the number of Indians and Malays interviewed is too small to permit meaningful quantitative analysis.[2]

## III. RESULTS

Neither sex nor religion discriminate respondents' rates of voting. The hypothesized relationship between age and participation was confirmed, i.e., voting rates increase until the middle-age category and decline thereafter. How-

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* Support was received from the National Science Foundation Grant No. GS-1361. Living expenses during field research were provided by Fulbright-Hays Fellowship No. FH 6-141.

[1] (Chicago: Rand McNally & Company, 1965).

[2] The ethnic composition of respondents in Kuala Lumpur and Penang compared with census distributions, by percentages, is displayed below. One can readily observe that Chinese predominate numerically in both cities. The number of Malays and Indians in each educational category is too small to permit a meaningful reporting of voter turnout rates for those categories.

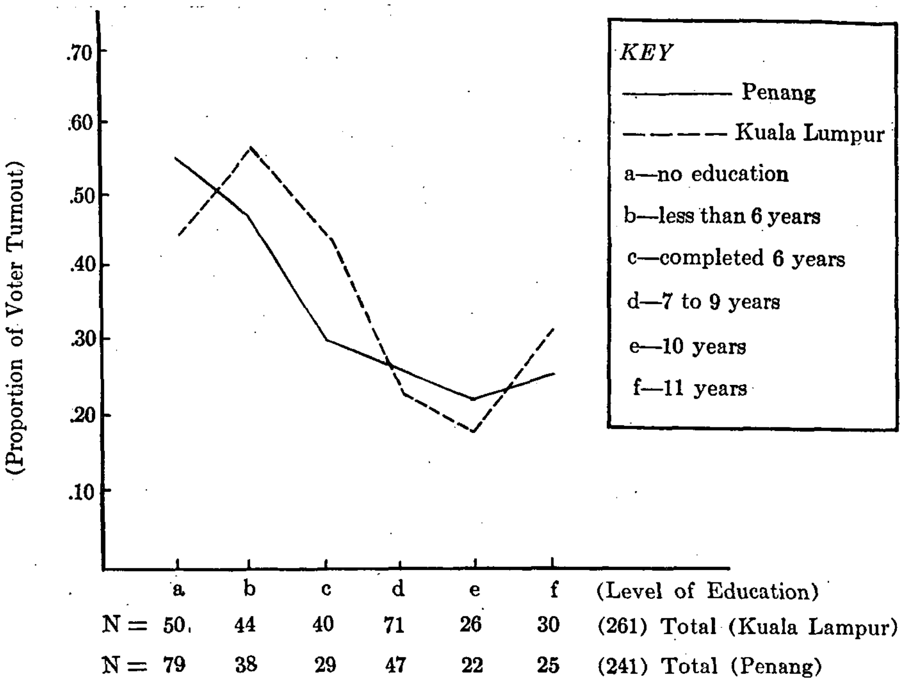
Kuala Lumpur

| Ethnic Group | ($N = 359$) Sample | ($N = 314{,}000$) Census (1957) |
|---|---|---|
| Malays | 13.3 | 15.0 |
| Chinese | 74.9 | 62.0 |
| Indians | 8.9 | 16.9 |

Penang

| Ethnic Group | ($N = 374$) Sample | ($N = 234{,}000$) Census (1957) |
|---|---|---|
| Malays | 16.8 | 11.4 |
| Chinese | 67.4 | 72.9 |
| Indians | 13.9 | 13.6 |

FIGURE 1. Proportion of Voter Turnout by Level of Education



ever, when education is plotted against rates of voter turnout among Chinese in urban Malaya, an unexpected result occurs. Figure 1 reveals this surprising outcome. Figure 1 shows that participation generally declines, and significantly so, as level of education increases. This result for 261 Chinese in Kuala Lumpur and 241 in Penang is not consistent with the hypothesized relationship presented in Milbrath. That the result appears for both cities, although slightly more pronounced in Kuala Lumpur, suggests that this decreasing relationship is not due to the uniqueness of a given Malayan city, but is generally applicable to urban Chinese voting behavior in Malaya.

## IV. DISCUSSION

The use of data collected in a transitional Asian society shows that the relationship between education and level of voter turnout differs from relationships previously reported in North American and Western European countries. One plausible explanation offered here is that increased levels of education among Chinese in urban Malaya lead to an awareness that one's vote is meaningless. I show elsewhere[3] that Malayan Chinese have been systematically discriminated against, disfranchised or otherwise reduced to a low level of political efficacy. Briefly, that article shows that Malay members of Parliament passed a constitutional amendment which potentially increases Malay representation by reducing rural constituency size to half that of urban constituencies. This is significant because Malays comprise a majority of the rural population while the Chinese are the majority of the urban population. As a result, the Chinese are at a comparative disadvantage in State and Parliamentary elections. Furthermore, non-Malay opposition political power was eliminated by the suspension of several important Chinese controlled municipal councils on charges of malpractice by Malay dominated State governments. This argument suggests that the better educated Chinese is more likely to realize his electoral powerlessness and, hence, he is less likely to vote.

In the business of writing lawlike generalizations, one lesson is clear. Comparisons limited only to advanced industrial societies in the "West" may provide somewhat misleading conclusions. Efforts should be made to generalize on a generic phenomenon such as participation using a broader range of geographical data.

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[3] See "The Manipulation of Ethnic Politics in Malaya," *Polity*, forthcoming (March, 1970).

# VOTING, OR A PRICE SYSTEM IN A COMPETITIVE MARKET STRUCTURE

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In this brief note it is demonstrated that if all the conditions for the existence of a competitive equilibrium are satisfied, then simple majority voting to determine the distribution of goods may be less efficient than a price system.

The argument here may be somewhat cryptic for those not familiar with the work of Anthony Downs.[1] A considerably more discursive presentation of the background material is given in "A Two Party System, General Equilibrium and the Voters' Paradox."[2] The tax and public goods aspect of the voting problem have been discussed elsewhere in "Notes on the Taxonomy of Problems Concerning Public Goods."[3] The result presented here, nevertheless, stands by itself, hence is presented in this brief form.

The political system is modeled at its simplest. We assume the existence of two players called "political parties." The goal of each player is to win an election by as large a vote as possible. A strategy for each player is to name a policy that it will carry out if it is elected. A policy is any point in the set of feasible distributions of final product. It follows immediately that, although any policy may be considered as a strategy, any non-Pareto optimal policies are dominated by some policy that is optimal. A discussion of the reasons for modeling a party in this simple manner is given in detail elsewhere. It is assumed that all voters are passive or "mechanistic," i.e., they do not form groups but merely vote individually, selecting optimally between the two policies offered.

Consider a three-person symmetric market. The three traders have initial endowments of $(a,0,0)$, $(0,a,0)$ and $(0,0,a)$. Each has the same differentiable utility function $\phi_1=\phi_2=\phi_3=\phi(x,y,z)$. It is easy to check that in this case the unique price system will be $(1,1,1)$ and the final imputation to each will be $(a/3,a/3,a/3)$. The worth of this imputation is shown as the point $E$ in Figure 1.

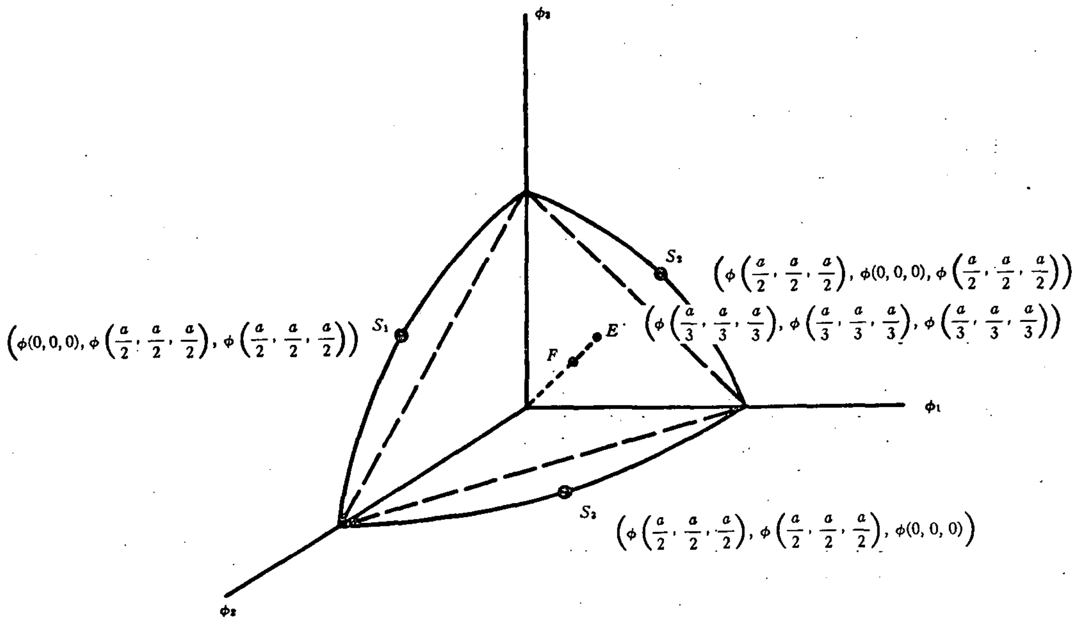
$\phi_3$
$\phi_1$
$\phi_2$
$S_1$
$S_2$
$S_3$
$E$
$F$
$\left(\phi\left(\frac{a}{2},\frac{a}{2},\frac{a}{2}\right),\phi(0,0,0),\phi\left(\frac{a}{2},\frac{a}{2},\frac{a}{2}\right)\right)$
$\left(\phi\left(\frac{a}{3},\frac{a}{3},\frac{a}{3}\right),\phi\left(\frac{a}{3},\frac{a}{3},\frac{a}{3}\right),\phi\left(\frac{a}{3},\frac{a}{3},\frac{a}{3}\right)\right)$
$\left(\phi(0,0,0),\phi\left(\frac{a}{2},\frac{a}{2},\frac{a}{2}\right),\phi\left(\frac{a}{2},\frac{a}{2},\frac{a}{2}\right)\right)$
$\left(\phi\left(\frac{a}{2},\frac{a}{2},\frac{a}{2}\right),\phi\left(\frac{a}{2},\frac{a}{2},\frac{a}{2}\right),\phi(0,0,0)\right)$

FIGURE 1

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[1] Anthony Downs, *An Economic Theory of Democracy* (New York: Harper & Row, 1957).

[2] Martin Shubik, "A Two Party System, General Equilibrium and the Voters' Paradox," *Zeitschrift für Nationalökonomie*, 28 (1968), 341–354.

[3] Martin Shubik, "Notes on the Taxonomy of Problems Concerning Public Goods," (Cowles Foundation Discussion Paper 208).

Suppose that instead of using a price system the society decided to adopt a simple majority voting procedure for the distribution of all goods. We must specify a condition concerning the protection of property rights of the minority. For simplicity we can either assume that any taxation up to confiscation of all possessions is sanctioned, or that any redistribution must satisfy conditions of individual rationality as defined by preferences and the distribution at the *status quo ante* the vote. For the purpose of the example we assume that any taxation is possible, hence an individual can end up with nothing. As is pointed out later, the result does not depend on this.

In Figure I, $s_1$, $s_2$, or $s_3$ could be obtained by a majority. They are not preferred to each other, yet one of them is preferred to any other policy that can be named. For example, suppose that the parties limited themselves to seven programs consisting of the three policies noted and the following four others $E$ and $((e,f,g), (g,e,f), (f,g,e))$, $((f,g,e)\ (e,f,g), (g,e,f))$, $((g,e,f), (f,g,e), (e,f,g))$ where $e+f+g=a$. Suppose that $e>f>g>0$ and $f>a/3$. The resulting $7\times 7$ matrix game is shown below (where 1 indicates a win by one vote, 0 a tie, and $-1$ a loss by one vote).[4]

$$\begin{array}{c|ccccccc|}
 & 1 & 2 & 3 & 4 & 5 & 6 & 7 \\
\hline
1 & 0 & 0 & 0 & -1 & -1 & 1 & 1 \\
2 & 0 & 0 & 0 & -1 & 1 & -1 & 1 \\
3 & 0 & 0 & 0 & 1 & -1 & -1 & 1 \\
4 & 1 & 1 & -1 & 0 & 1 & -1 & 1 \\
5 & 1 & -1 & 1 & -1 & 0 & 1 & 1 \\
6 & -1 & 1 & 1 & 1 & -1 & 0 & 1 \\
7 & -1 & -1 & -1 & -1 & -1 & -1 & 0 \\
\hline
\end{array}$$

This game has a mixed strategy solution of $(0,0,0,1/3,1/3,1/3,0)$ for each party. As is to be expected, each names the same (mixed) strategy and each stands the same chance of winning the election.

A possible interpretation of the mixed strategy is that the majority rule emphasizes the diversity of interests—any majority wants to benefit by taxing the minority. As this situation is symmetric, the parties wish to appear to be all things to all men at the same time; hence, they mix their strategies over different nonsymmetric outcomes favoring different interest groups.[5]

The individual consumer obtains $1/3(\phi(e,f,g)+\phi(g,e,f)+\phi(f,g,e))$ from this policy. However, if he is risk-neutral or risk-averse, then from the assumption that he has a convex set of preferences $\phi(a/3,a/3,a/3)\geq 1/3(\phi(e,f,g)+\phi(g,e,f)+\phi(f,g,e))$. This is shown as the point $F$ in Fig. 1. If his preference set is strictly convex, then the midpoint solution that is the outcome of the competitive market is strictly preferred to the outcome obtained through the voting procedure.

Had there been a limitation on taxation, the same result would hold with the modification that some of the more extreme policies which discriminate against minorities would be limited.

The result did not depend upon selecting a finite number of policies for the two parties to offer. If all points on the optimal surface are available, then the strategic problem of the parties becomes equivalent to that of a continuous Blotto game, which has been solved by Gross and Wagner.[6] All of the solutions place a zero probability density on the center.

It might be thought that the result depends strictly upon the symmetry of the preferences of the voters. This is not the case. If they are risk-neutral or risk-averse, then whenever the "political noncooperative game" has no pure strategy solution, the result will not be Pareto optimal. Depending on the degree of symmetry and the level of risk aversion, the competitive equilibrium point will lie above the expected value of the vote in a zone of the Pareto optimal surface such that everyone is better off at the competitive equilibrium. This is shown in Figure 2. $V$ is the expected value of the outcome obtained by voting and $E$ the competitive equilibrium. As long as $E$ lies between $V^1$ and $V^{11}$ the result is true. This diagram was drawn for ease of exposition in two dimensions where $P_1$ and $P_2$ are the evaluations of Voters 1 and 2. The picture is basically the same for higher dimensions.

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[4] Two models can be considered, the first in which a party's goal is to win by as large a vote as possible, the second where its goal is just to win. In this example, the latter assumption appears to be the more reasonable.

[5] Kenneth Arrow has pointed out that one must distinguish between the two cases where (1) the parties announce their (mixed) strategies to the public, or (2) randomize first and announce the the results of the randomization as their policies to the public. If we adopt the first interpretation, we may view the mixed strategy as being a "degree of belief" in the mind of the voter. He perceives some of the contradictions in the statements of the parties; hence he has only a tentative view of what their actions will be. In the second instance we assume that the parties polarize the issues for the voters. In the first case we must assume a cardinal utility scale for the voters who evaluate uncertain outcomes.

[6] O. Gross and R. Wagner, *A Continuous Blotto Game* (The RAND Corporation, RM-408. June 17, 1950).

A possible interpretation of the general result is that if in some sense the original distribution of resources is regarded by the voters as more or less equitable or symmetric, the economic process keeps this property; whereas the political process introduces the possibility of considerable asymmetry as the majority takes from the minority. Without special laws to protect the minority and since anyone could be in the minority, all participants evaluate the expected worth of the mix of outcomes offered by the voting process as worse than the economic outcome.

In the continuous case the policies which are not used are those which give a single individual more than two-thirds of all of the gain. A coalition of the remaining two can always be effective against such an extreme division. The midpoint or even split is also ruled out because any two players can improve their lot by taking from the third.

This model is obviously highly unrealistic and restrictive. Nevertheless, the result is possibly instructive and has a useful interpretation. The outcome is Pareto optimal, but the expected outcome is not. The loss of efficiency appears to be related to the power struggle and the inherent favoring of a nonsymmetric solution by the majority in a political situation. The economic mechanism, *given* the initial distribution of resources is anonymous and egalitarian.

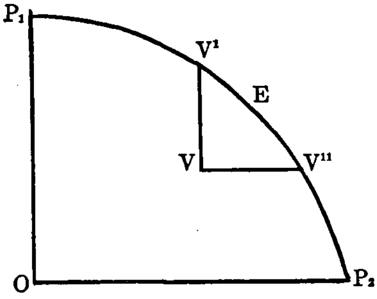

FIGURE 2

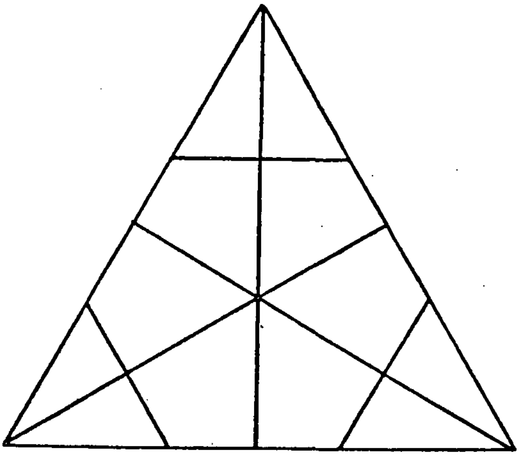

FIGURE 3

It must be stressed that usually a reason for using a voting system is that no price system exists. This is almost always the case when the production of public goods is called for.

## APPENDIX

One of the solutions given by Gross and Wagner is:

Colonel Blotto might do the following: He plays a continuous density function $p$ over the regular hexagon (of Fig. 3), i.e. if $d\sigma$ denotes an element of area at point $x$ within this hexagon, the probability that he chooses a point within $d\sigma$ is given by $p(x)d\sigma$. The density function $p$ is determined as follows: $p$ is constant* on the perimeter of the hexagon, zero at the center, and linear along any straight line segment joining the center with an arbitrary point on the perimeter.

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* Normalization reveals the value of this constant to be $9/4\sqrt{3}$.

# COMMUNICATIONS

To the Editor:

Barrington Moore, so the editor informed me, was to be provided with the opportunity to respond to my critique with an essay up to two thirds the length of my original article. He chose to limit his comments to slightly more than seven typewritten pages and to discuss only three (four?) of the many substantive issues raised, while taking considerable space to attack my brief concluding comments as to the possible underlying assumptions of his book. The editor has allowed me 700 to 800 words to conclude this exchange. While I know that I can rely upon his generosity, I will be as succinct as possible.

Moore is disturbed by my characterization of his intellectual development. I did assume that the remarks he quotes were autobiographical. I did not, however, call him a "fellow traveller," nor did I neglect to point out that his view of the Soviet experience was as negative as his view of gradualism. (I think he ascribes the use of the term "fellow traveller" to me for quite specific reasons, which I shall return to later.) I still think my interpretation is correct, for it reinforces my memory of classes of his which I attended in the early 1950's, in which he indicated what his sympathies had been earlier. (His remarks about books of his published in the 1950's are, therefore, without relevance.) However, the point is a very, very minor one and I shall not belabor it.

Moore rejects the appellation "neo-Marxist." I define what I mean by the term as applied to him quite precisely in terms of two propositions which I think underlie his analysis. If he does not like the term I will call him anything he wishes. I still think that I am correct, despite his "explicit disclaimers." The reader will have to judge for himself. Moore also denies that he used the expression "non-repressive society." I never asserted that he did. I did argue that his analysis, since he fails to distinguish among degrees of exploitation in the societies which he examines, and since he strongly implies that socialization into all past societies has been uniformly brutal (see the citations and quotes in my paper), only make sense if one assumes that this is his assumption. If I am wrong then many of the more pithy phrases he uses are mere rhetoric, and the book is a far less substantial work than I thought it was.

Moore also denies that he is critical of Max Weber. I never said that he was critical of all of Max Weber's work. I did assert that he was critical of Weber's belief in the autochthonous causal efficacy of values, especially of religious values, and of Weber's emphasis on the role played by religious ideas in the development of European capitalism. Moore's sharp disagreement with this view and the view of those scholars (such as Robert Bellah) who start from a similar perspective is quite obvious in the book as a whole and on the pages I cite.

As to the substantive points: Moore claims that he never asserted that the Civil War was necessary if a coalition of northern business and southern Junkers was to be avoided. I refer readers to the chapter itself and to the whole thrust of the book, but let me quote some passages on the pages I cite, lest the issue be in doubt:

> German experience suggests that, if the conflict between North and South had been compromised, the compromise would have been at the expense of the subsequent democratic development in the United States. (p. 115)
>
> Such an alignment would have been of Northern industrialists and Southern planters against slaves, small farmers and industrial workers. This is no abstract phantasy. Quite a few forces pushed in this direction before the Civil War . . . . any peaceful solution, any victory of moderation, good sense and the democratic process, would have had to be a reactionary solution. (pp. 131–132)
>
> In the United States plantation slavery was an important aspect of capitalist growth. On the other hand, to put it mildly, this was an institution unfavorable to democracy. The Civil War overcame this obstacle, though only partially. (p. 421)

Second, Moore argues that our differing interpretations of Mary Wright's book are merely differing interpretations. Not so. It is perfectly proper for a scholar to draw supporting evidence from a book with a thesis contrary to his own. It is *not* legitimate for him to cite the book as supporting his thesis. Moore cites Wright as supporting his contention that, during the Restoration period, reform was blocked entirely by class interests. While Wright certainly does say that class interests played a role, she emphasizes cultural variables, as, for example: "Third, and most important, Restoration leaders did not spend much time thinking about commerce, not even about its evils. . . . Commerce was not worthy of systematic attention." (Wright, p. 170). Time and time again, Moore draws implications which are not legitimate from his sources. Indeed, in my essay, I did not cite all the instances which could be cited.

The adequacy of Moore's comparison of the role of the Chinese and Japanese gentry and my critique readers will have to judge for themselves. His comment is too general and the issues too complex for an extended discussion here. I do stand corrected on one point. The word is "exploitative" not "exploitive." I shall never forgive my "helpers" for missing that one.

Moore's use of Beals (along with others) is designed to prove that large landlords in India exploit to the limits of their ability and are opposed to innovation. He rejects my citation as not to the point, and argues that, on the very same page I cite, Beals supports him. Let me quote in detail from the pages in question.

> Landlords are the educated men of their villages, the innovators who introduce new agricultural techniques, the protectors who alone are capable of dealing with police officials and settling conflicts. . . Many villages have irrigation reservoirs, wells, schools, and other improvements because the landlords were able to influence government officials. Although people in Gopalpur would be delighted at an opportunity to divide their Gauda's property among themselves, the prospect of their being no Gauda whatsoever fills most people with dismay.
>
> The desire of government officials to raise the status of jatis which have been condemned to follow the more menial and less remunerative occupations occasions even more doubts. A farmer cannot weed and harvest his crop without labor. If those who now serve as laborers are converted into farmers there will be no one to do the weeding and harvesting. . . . People in Gopalpur are told that they must stop discriminating against people in low-ranking jatis, but they are not told what to do in order to obtain the services formerly performed by them. (Beals, pp. 82–83)

As is quite clear from this quotation and a reading of Beal's book as a whole, not only do large landowners (Gaudas) perform services which are not simply in their self interest narrowly defined, but the views ascribed to large landowners by Moore are actually ascribed by Beals to "people in Gopalpur," i.e., the small farmers and shepherds who make up the bulk of the population.

Both Beals and I are for land reform and a shift in the structure of power. Neither of us (for I accept his analysis) believes that large landowners act purely or even primarily out of altruism. Indeed on page 78 Beals does point out that large landowners have no incentive to lend new brands of seed to those who borrow from them because they will gain no advantage from this and will be blamed if the seed fails.

This, however, is not the point. The point is that nowhere does Beals ever imply that the large landlords are merely interested in maximizing exploitation, or that the problems faced by India are merely or even primarily the result of exploitation. Rather, as he notes, the problems faced by Gopalpur and the difficulties of reform stem from the fact that: "The culture of Gopalpur is an organic whole; its religion and its social organization are adapted to the economic tasks traditionally carried out in the village." (Beals, p. 83).

One final point. I have, in the course of my academic writing, criticized other scholars in much the same vein that I have dealt with Moore's book. Some have responded, as does Moore, by questioning my craftsmanship (a legitimate ploy), but none has attacked my motives by implying that I was a member of the establishment attempting to stiffle dissent by conducting what amounts to a "Congressional inquiry" masked by a "scholarly facade." This is an effort to dismiss a critic by accusation, because the expected academic reaction is one of horror and contempt. Thus it serves much the same function in the academic world that accusing a person of being a Communist did for segments of the non-academic world in the 1950's. This kind of "left-wing McCarthyism," to use I. F. Stone's phrase, has become increasingly common of late, and threatens to reduce the possibility of reasoned intellectual discourse.

It is hard to know how others will interpret the words one puts on paper. While I was sharply critical of Moore, I do not think that I suggested he was someone else's man or motivated by some secret destructive purpose. In short, I assumed his intellectual integrity. I regret that he has not been able to respond in kind.

*Smith College*

STANLEY ROTHMAN

TO THE EDITOR:

Simply to keep the record straight, I should like to comment on a minor (to most readers no doubt trivial) point in Professor Wolin's "Political Theory as a Vocation" (this REVIEW, December, 1969, pp. 1062-1082).

Professor Wolin remarks: "It is excusable that a decade ago a political scientist could contend that only a 'fanatic' would want to 'maximize' political equality and popular sovereignty at the expense of other values, such as leisure, privacy, consensus, stability, and status." Presumably because this egregious assertion is "excusable," Professor Wolin generously displayed forebearance in not citing the source, which is my *A Preface to Democratic Theory* (1956), pp. 50-51.

Because the needs of condensation and forceful prose have produced a one-sentence summary that may cause many readers to misinterpret my argument—as quite possibly Professor Wolin himself has—I take the liberty of appending the fuller text. What I intended to say, and so far as I can read my own words accurately, what I did say, was that no one, except perhaps a fanatic, would *completely* sacrifice *all* other values to any two political goals, even two goals as lofty as political equality and popular sovereignty. In the interval since I wrote that book I have changed my mind about a great many things, but not about this. I cannot believe that Professor Wolin means to imply that he would himself sacrifice *all* other values to gain these two ends. For my part, happily, I have never run into anyone who by the testimony of his own life and activity demonstrated that he really meant to do so, and I am quite sure this applies to Professor Wolin also. Were I to encounter such a person, I think I would be justified in regarding him as a political fanatic, or at any rate as a person with an exceedingly narrow vision of the good society.

That some people sometimes write as if they would act in this way is to me only further evidence that political rhetoric invariably outruns political commitment.

*Yale University*

Robert A. Dahl

To the Editor:

My reason for not attributing the paraphrase to Professor Dahl was the one which he hints at. I was aware of recent statements which represent a change in his viewpoint, but I was not certain about the extent of the modifications. As the context makes clear, I thought it typified a widespread opinion. If he has been offended, I sincerely apologize.

The remainder of his letter raises issues which cannot be dealt with thoroughly in this forum. Perhaps a few observations are appropriate. First, although it is awkward to dispute claims based upon personal experience, which is why it is considered bad scientific and philosophic form to argue in this manner, Professor Dahl's formulation, that he has never encountered anyone who would sacrifice "*all* other values" to gain the values of political equality and popular sovereignty, leaves me puzzled on other grounds. Does "completely sacrifice" mean to destroy or wholly surrender something for something else so that the thing destroyed can never be sought as long as the preferred thing is still preferred? If so, then, of course, the fanatic does not exist who would "completely sacrifice all other values to any two political goals," because, at the very least, he would retain the value of life, i.e., he would devote time to eating and sleeping. Such a concept of a fanatic has been best described by Professor Dahl's language: it "tells us nothing about the real world. From it we can predict no behavior whatsoever." (*Preface*, p. 51.) I doubt, therefore, that the concept is of much help in detecting the hypocrisy alleged to exist between "political rhetoric" and "political commitments." Perhaps this explains why, for the most part, Professor Dahl's fanatic in the *Preface* appears more pliant than the fanatic in his present communication. The latter seems rigid and obsessive ("*completely* sacrifice *all* other values"), the former more accommodating, unwilling to sacrifice completely, preferring to maximize, to calculate costs, and to accept incremental gains.

Perhaps Professor Dahl
has changed his mind after all.

Second, what interested me about the passage as a whole (*Preface*, pp. 50-51) was not the relative merits of fanatics and marginalists, but the way in which the problem was formulated and what was selected for contrast. It seemed revealing to me that the word "fanatic" should come to mind in the course of a contrast between two strongly political values (political equality and popular sovereignty) and an assortment of values associated primarily with social, private, or material matters ("leisure, privacy, consensus, stability, income, security, progress, status. . . ."). There is a certain bland and unprovocative quality about each of these items and it requires some effort to imagine any of them arousing the passion associated with the dictionary definition of fanatic, "too great enthusiasm." A world may be congealed in the choice of a word. Accordingly, the formulation seemed to me to belong to the same family of assumptions which support a pluralistic, bargaining conception of society and to explain in some measure why many political scientists have not been particularly alarmed at the apathy of the American citizenry. It is the nature of "us marginalists" to characterize all political enthusiasts as fanatics or deviants.

Perhaps, as Professor Dahl notes, "most of us are marginalists" and perhaps "it is an observable fact that almost no one regards political equality and popular sovereignty as worth an unlimited sacrifice of these other goals," but it is also an observable fact that over the past decade a significant number of black leaders, draft resisters, and politically active students have acted, not as marginalists, but as marginal men determined to challenge the dominant assumption which identifies rationality with privacy, etc. and fanaticism with equality, etc. The Latin root of fanatic also means "inspired by a deity."

Sheldon S. Wolin

*Center for Advanced Study*
*in the Behavioral Sciences*

TO THE EDITOR:

Let me add a third point of view to the two discussions of my book *Public Policymaking Reexamined* (this *Review*, September, 1969, pp. 915–921).

My basic disagreement with both reviewers concerns their frame of appreciation, which is "normal" political science. (I am using the terms "normal," "paradigm," and "scientific revolution" as proposed in *The Nature of Scientific Revolution* by Thomas S. Kuhn, Chicago: University of Chicago Press, 1962.) My major endeavor is to try and work out a new paradigm of "policy sciences" to help face the major crisis of irrelevance for acute human concerns facing contemporary political science. The main concept expressing the essential concern of my book is *meta policy,* that is policy on how to make policy. My main claim is that meta policy is a critical variable in determining the quality of concrete policies, that meta policy can be studied, that meta policy can and must be improved, and that the study and improvement of meta policy should serve as the foci of a new interdiscipline of policy sciences, based in part on a transformed political science. To accept the analogy used by the second reviewer, though not his conclusion, I think we must improve the "engine" of policymaking as such. Without minimizing the need for other approaches, I do think that a "policy science" paradigm is essential for increasing the contributions of knowledge and science to human affairs, and I try in my book to explore some of the new frameworks, concepts and theory needed for that purpose.

It is a pity that the reviewers did not squarely face my efforts in that direction. They take up implications of my approach rather than the approach itself. For instance, I agree that detailed application of my ideas to concrete policy areas will be necessary, but exploration and explication of new basic concepts and general theory must come first. For my purposes most available empiric studies of policymaking (however valuable from the point of view of "normal" political science) are indeed too narrow and lack a comprehensive theoretic foundation. (I am sorry, but I think the bibliographic essay at the end of the book, however selective and by now outdated, precludes the defense of "lack of familiarity" kindly suggested by the first reviewer.) To sharpen the differences between the first reviewer and myself: I reject the criterion whether my "proposals fit into the empirical literature . . . and prescribe a set of goals that are consistent with existing knowledge" (p. 916). The correct test of my approach should be whether it opens up promising vistas for novel knowledge and action. To illustrate: From my reading of the new emerging paradigm of policy sciences, the "normal" conception of basic different roles of the academic and the practitioner (put forth by the first reviewer on p. 918) is unacceptable; instead, we must develop a new type of policy oriented scientific role, to be fulfilled by a new class of academics and based on innovative knowledge.

Evaluation by "normal science" is at least legitimate (by definition), though inappropriate, missing the main thrust of my book and failing to face issues of dominant importance for the future of political science and politics. What is completely unacceptable, even by the tenets of "normal political sciences," are failures to understand the elements of the book and misquotations in discussing them—as done by the second reviewer. To illustrate:

1. A major part of my book is devoted to evaluation and presents a set of operational secondary criteria correlated with net output and therefore helpful for evaluating policymaking and policymaking-improvement in terms of impacts on reality—indirectly, but in a useful and the only feasible way. The second reviewer fails to understand this structure, quoting three of the involved logical steps (on page 920), but ignoring the fourth and critical one: While net output can almost never be evaluated directly, *it can and should be reliably evaluated through an explicit set of secondary criteria.*

2. I agree that the title headings of some of my improvement suggestions are not exciting (not having been written for a mass audience newspaper). But each one of these titles opens a discussion of concrete proposals, with substantive contents, ignored by the reviewer. Typically, the one improvement suggestion regarded favorably by the second reviewer, namely redundance, is presented by him as unprovable. This is a mistake, as the "fail safe" functions of redundance can be proven even mathematically, as explained in the book (page 211 and note 11 there).

Let me conclude by presenting a scheme for the evaluation of book reviews, illustrating the distinction between primary and secondary criteria. It is up to the reader to judge, after examining my book, whether my parameters are correct.

YEHEZKEL DROR*

*The RAND Corporation*

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* Any views expressed in this communication are those of the author. They should not be interpreted as reflecting the views of the RAND Corporation of the official opinion or policy of any of its governmental or private research sponsors.

### SIMPLIFIED SCHEME FOR EVALUATION OF BOOK REVIEWS

(Applied to the reviews of *Public Policymaking Reexamined*)

| *PRIMARY CRITERION* | *Review One* | *Review Two* |
|---|---|---|
| Extent of help to reader in deciding whether to read a book and how to read a book; extent of help to author to improve his thinking | Note: This primary criterion is difficult to use. Therefore, a set of secondary criteria must also be used, which should satisfy two conditions: (1) They are correlated with the primary criterion; (2) They are operational. | |
| *SECONDARY CRITERIA* | *Review One* | *Review Two* |
| Book has been closely studied by reviewer | Yes, with exception of Bibliographic Essay | Doubtful |
| Book evaluated in terms of normal science or (when applicable) new paradigms | Normal Science | Normal Science |
| Review shows understanding of the book and its main argumentation | Narrow Understanding | Parts Yes<br>Parts No |
| Review provides correct summary of main ideas | Partly | No |
| Review deals with overall *Gestalt* of the book, in addition to particular elements | No | No |
| Review provides intelligent discussion of particular elements of book | Yes, within a conservative frame of appreciation | No |
| Review avoids mala fides and personal hostility | Yes | Yes |
| Review avoids misleading and out-of-context quotations | Yes | No |
| Review avoids misleading side remarks and innuendos | No | Yes |
| Review avoids main preoccupation with the reviewer's own pet subjects | Yes | Yes |
| Overall evaluation in terms of cluster of secondary criteria | Fair, within the framework of a traditional approach; fails to consider challenge of new paradigms | Sub-standard |

# BOOK REVIEWS AND NOTES

BOOK REVIEWS

*Kalenskii*, Politicheskaia nauka v SSha: Kritika burzhuanykh kontsepsii vlasti. (Political Science in the USA: A Critique of Bourgeois Conceptions of Power) PETER H. SOLOMON, JR. . . . . . 188

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# BOOK REVIEWS

*Politicheskaia nauka v SShA: Kritika burzhuanykh kontsepsii vlasti.* (Political Science in the USA: A Critique of Bourgeois Conceptions of Power). BY VALERII GEORGIEVICH KALENSKII. (Moscow: Iuridicheskaia literatura, 1969. Pp. 103.)

When Soviet scholars began to conduct empirical sociological research at the beginning of the 1960's (for the first time in three decades), they devoted their first monographs to criticism of Western "bourgeois" sociology.[1] Now in 1969, when the birthpangs of political science are being felt (simultaneously with the founding of the section for the study of social-political phenomena at the new Institute of Concrete Sociological Research in Moscow), there has appeared in print the first serious Soviet study of American political science. Like his predecessors in sociology, Kalenskii aims not only at providing information about areas of inquiry and methodologies current in the West, but also at helping to legitimate the new area of study in the U.S.S.R. by demonstrating its potential contribution to the ideological struggle with the West.

In a relatively short survey Kalenskii succeeds in communicating a great deal about American political research today and about its intellectual origins. He combines both focus and breadth in his pithy restatements of the main ideas and findings of the literature he discusses. Given the difficulty in obtaining Western publications in the Soviet Union, I find impressive Kalenskii's choice of important and representative examples of the ideas he discusses.

The book consists of three long chapters and a conclusion—one chapter on the origins and history of American political science, especially the growth of behavioralism; a second chapter on the concept of power, interest group theory, pluralism and elites; and a third chapter on political socialization and participation, political culture, legitimacy and alienation. A list of authors whose works are discussed in detail includes: Merriam, Lasswell, Easton, Bentley, Truman, Schattschneider, Hunter, Dahl, Mills, Lane, Kornhauser, Lipset, Almond, Verba, Mathews, and Murphy.

In the tradition of the sociology of knowledge Kalenskii explains the rise of political behavioralism in terms of changes in the social environment. Like Western analysts (e.g. Eckstein) Kalenskii asserts that the disintegration of European democracies into fascist regimes and the consequent development of elitist political theories stimulated empirical study of American democracy (p. 9); also, that the replacement of "traditional American isolationism" by "the unprecedented foreign policy expansion of American imperialism, actively interfering in international political life" (p. 20) (it might have been put more delicately) stimulated sociological analysis of the international political situation and the political systems of foreign countries.

A sociology of knowledge approach also influences Kalenskii's *critique* of American political science. Usually, he judges the underlying value biases or normative assumptions of the social scientist, as evidenced in his research or in his reaction to the results of his research. Rarely does Kalenskii consider a work on its own grounds, by examining the accuracy of the research or the appropriateness of the conclusions. (The main internal criticisms deal with semantic or conceptual problems, such as Easton's blurring of the difference between social and political power (p. 27) and Lipset's somewhat strained attempt to distinguish between political science and political sociology (p. 14) ). In short, the critique is primarily ideological.

Ideological critique is often useful in delving behind research and providing a larger perspective from which to view it. When exaggerated, however, such critique leads to simplicity, dogmatism and distortion. Kalenskii's ideological critique of American political science consists both of the constructive and the exaggerated varieties. For example, the author rightly points to the primary concern of Almond and Verba (in *The Civic Culture*) (p. 82) and of David Truman (in *The Governmental Process*) (p. 35) for the stability of the democratic polity; to the general bias of functional analysis in this direction; and to the classic approval of political apathy by Berelson *et al.* in one of the early voting studies. (p. 67). Yet, after describing in detail Donald Mathews' *U.S. Senators and Their World*, "a very serious positivistic analysis of the problem of leadership," Kalenskii condemns the author for failing to criticize his finding of a predominance in the Senate of men from middle or upper middle class origin. (p. 59). In the case of Richard Merelman's article on the use of symbols in political legitimation, Kalenskii sees only dangerous manipulation; "this recommendation of speculation on democratic slogans is a clear illustration of an approach to problems of political life so characteristic of contemporary positivistic American political science, which pretends at objectivity and ideological neutrality. The narrow class basis and apologetic nature of such recommendations is completely obvious." (p. 79).

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[1] See G. N. Osipov, *Sovremennaia burzhuaznaia sotsiologiia—kriticheskii ocherk* (Moscow, 1964); G. M. Andreeva, *Sovremennaia burzhuaznaia empiricheskaia sotsiologiia—kriticheskii ocherk* (Moscow, 1965); I. S. Kon, *Positivizm v sotsiologii; istoricheskii ocherk* (Leningrad, 1964); F. M. Reshetnikov, *Sovremennaia amerikanskaia kriminologiia* (Moscow, 1965); Iu. Zamoshkin, *Krizis burzhuaznogo individualizma i lichnosti; sotsiologicheskii analiz nekotorykh tendentsii v obshchestvennoi psikhologii SShA* (Moscow, 1966).

Fixation on ideological critique leads at best to a valid but narrow kind of criticism which omits such important evaluative considerations as the fit with reality, so appropriate in dealing with *empirical* theory. In the Soviet context, however, critique of this kind has the advantage that by focusing on the researcher rather than the research itself, it leaves open the possibility of similar research being carried out in the USSR, albeit in a different spirit! And indeed, the research least scathed by Kalenskii is the highly empirical work (e.g. Matthews, Lipset, Murphy) most akin to research presently feasible in the U.S.S.R. Just as empirical research without accompanying middle level propositions has been characteristic of the first decade of "concrete sociological research,"[2] so the first genuine political science research is highly empirical. Typical is the research conducted recently by the sociological laboratory of the Institute of State and Law on the activities of deputies to local soviets.[3] In the near future one may expect other studies of relatively powerless institutions, those whose concerns are with primarily symbolic functions or local regulatory ones; and perhaps, studies of leadership or decision-making in social and economic institutions.

In his study Valerii Georgievich all but explicitly welcomes political research in the Soviet Union. It is not accidental that he stresses the role of the "natural process of integration and differentiation characteristic in the development of science" in the emergence of political research in the USA and then draws an analogy with two fields of social science which have only recently been established in the USSR, criminology and management science. (p. 4) In addition, Kalenskii emphasizes the usefulness of political science research to American political leaders (p. 61); by implication it could help Soviet leaders as well. Furthermore, Kalenskii states that "many facts of political reality brought to light in the course of empirical research can and must be used by Marxist scholars studying the political organization of capitalist society." (p. 98).

From a Western point of view Kalenskii has offered no original criticisms—his most valid and interesting ones Western scholars have offered before. Appropriately, Valerii Georgievich ends the book by citing the perceptive critical articles of Christian Bay and Jack Walker which appeared on the pages of this journal (APSR, March 1965 and June 1966) (p. 101). He has succeeded, however, in providing the Soviet reader with a clear picture of the range of American political science and of its main issues and ideas, at least in the area of democratic politics. This is no mean accomplishment.

PETER H. SOLOMON, JR.
*Columbia University*

*Children in the Political System: Origins of Political Legitimacy*. BY DAVID EASTON AND JACK DENNIS. (New York: McGraw-Hill, 1969. Pp. 440. $9.75.)

This is the second major volume to come out of the "Chicago" study of political socialization among elementary school children. The first volume (Robert D. Hess and Judith V. Torney, *The Development of Political Attitudes in Children,* noted in this *Review,* December, 1968) was written more from the point of view of the psychology of child development. In the present volume Easton and Dennis analyze the same data from a political science perspective, and particularly from the viewpoint of systems analysis.

The empirical base for the book is over 12,000 questionnaires filled out by children in grades 2–8 in eight cities across the United States. The analysis is limited by the inclusion of only white, urban children and by the single time period covered. These limitations are undoubtedly excusable in an initial study, however, and while not avoiding generalizations, the authors extend their findings with considerable care.

The focus of the book is exclusively on children's views of political authority—primarily the President and policeman, but also including less obvious personalities (the average Senator), institutions (Congress and the Supreme Court) and more abstract objects (government). These objects are studied for their visibility and salience to children, and for the children's cognitive and affective images of them. The chief findings are conveniently summed up by the authors under four headings (pp. 391–2): politicization, personalization, idealization, and institutionalization. Politicization refers to the fact that young children learn to distinguish between family authority and a power external to and superior to the family. Personalization means that children become aware of political authority first and most easily via contacts with individuals. This leads to the "head and tail" effect, wherein youngsters learn first about the extremes of President and policeman. Idealization of authority occurs early in life, as political figures are viewed as extraordinarily benign. This halo, of course, wears off as children progress through the elementary grades. Finally, institutionalization refers to the process of shifting one's attention and affection from individuals to political institutions.

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[2] See especially the critique of early sociological studies by V. A. Iadov, "Rol metodologii v opredelenii metodov i tekhniki konkretnogo sotsiologicheskogo issledovaniia," *Voprosy filosofii,* 1966, No. 10.

[3] "Effektivnost deputatskoi deiatelnosti (Opyt konkretnogo sotsiologicheskogo issledovaniia na materialiakh Armianskoi SSR)" *Sovetskoe gosudarstvo i pravo,* 1969, No. 1.

For each of these features of early socialization, Easton and Dennis discuss in detail the implications for the political system. It is interesting in this connection that they conclude that three of the four fundamental processes are probably universal among "persistent" (carefully distinguished from "stable") political systems. Politicization, personalization, and institutionalization are features of socialization nearly everywhere, but "hostilization" may occur when the government is not perceived as benevolent.

Most of the book is concerned with aggregate response patterns and changes over the grade school years. In two chapters, however, an interesting but mostly unsuccessful attempt is made to find predictors of the children's images of authority. A multivariate analysis in Chapter 17 shows grade, city, partisanship, and religion to be among the best predictors of children's feelings. But in all cases the relationships are very weak. Some minimal support is also found for the psychoanalytic hypothesis that ratings of political authorities are transferred from comparable ratings of the father. The reason for their limited findings is not at all obvious, but one wonders whether it is in large part due to the fairly low stability of children's responses (reported in Hess and Torney, pp. 238–40).

The book's chief virtue lies in relating the findings about early socialization to the persistence and stability of political systems. That this is done expertly, there can be little doubt. However, whether the work contributes to the enhancement of political theory, as the authors wish, is partly dependent on how one judges systems analysis. Regardless of one's view of that approach, a positive feature is the emphasis on change as well as stability of political systems. This is done not only by arguing that systems analysis is not bound to the status quo. Rather, adult ratings of policemen are specifically introduced to show at what age individuals are most likely to contribute to change in the political system. (The findings point to the early adult years.) In addition, the term "hostilization" is coined in order to emphasize the non-universal nature of American children's positive views of authority and to stress the possible impact of alternative affective images.

Another positive feature of the book is that the authors avoid the notable pitfalls of the Hess and Torney volume. It is made clear, for example, that however much change takes place during the elementary school years, socialization continues on into the adult life-span. In fact, for some orientations, socialization may just be beginning at adolescence. Similarly, the unwarranted judgment of the primacy of the school is absent in the Easton-Dennis work. However, perhaps to avoid this problem, teacher's responses are often left out, even though with careful interpretation they can be quite useful.

A possible defect is the book's narrow focus on "system politics," which prohibits the inclusion of data on participation, partisanship, party differences, voting, etc. Ordinarily this defect might be more critical. Here it means that the Hess and Torney volume must be looked at as complementary. In particular, the material in Chapter 4 of that book helps expand on the subject matter of the present work. Moreover, on the basis of Hess and Torney's findings in their Chapter 7, it would be interesting to see the results of Easton and Dennis' multivariate analysis on topics other than images of the President and policeman.

Another unavoidable drawback of the book is that much of the data now seems redundant. After reading Hess and Torney, Greenstein, and various articles by Easton and Hess, the findings will not surprise anyone. Perhaps because of this, the book seems overly long in many places. For a similar reason, particularly the overlap with Easton's other writings, the first hundred pages seem excessively drawn out. The book is at its best in the later chapters when data on adult views of authority are added, the multivariate analysis is undertaken, and "cross-system" implications are considered.

An omission which might have added some freshness to the analysis is to see whether some children are ignorant and/or alienated about most objects of political authority while others are knowledgeable or positive about nearly all objects. It is argued in Chapter 13 that there are multiple "open doors" through which children can become aware of and supportive of political authority. But nowhere is it seen whether children's views of one political authority are correlated with perceptions of other political figures. This could happen, of course, even when the aggregate responses about authorities are rather different. Along the same lines, the relationship between cognitive and affective images could be explored empirically. The aggregate development suggests that the more knowledgeable students become about politics, the less ideally they picture it. Does this occur at an individual level within each grade? Or do greater knowledge and less idealization covary simply because they are both functions of age and experience?

In spite of these criticisms, the Easton-Dennis book helps round out the reporting of the Chicago study by tying in the findings on early socialization to the development of support for political authority. In doing so, it has gone further than most studies in discussing the implications of empirical work for the functioning of political systems.

RICHARD G. NIEMI
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*Violence in America: Historical and Comparative Perspectives,* A Staff Report to the National Commission on the Causes and Prevention of Violence. EDITED BY HUGH DAVIS GRAHAM AND TED ROBERT GURR. (U.S. Government Printing Office, 1969. 2 vols. Pp. 644. $1.25 each.)

This Report to the National Commission on the Causes and Prevention of Violence contains twenty-three papers on various aspects of violence by historians, humanists, and social scientists. The papers are grouped under nine general topics, including historical and comparative accounts of violence in Europe and America; the effects of immigration, the frontier, class, and race as factors contributing to violence in America; characteristics of crime in the United States; patterns of domestic and international strife; the process of rebellion; and ecological and anthropological perspectives. Two basic conclusions emerge: first, violence is a common recurring condition in all parts of the world, during all periods of history, at all stages of social development; and second, no coherent analytic framework exists within which the causes, processes, or consequences of violence can be understood or even systematically described.

The belief that violence is a pathology has a profound hold upon us. Biblical prophecies of a harmonious golden age; the Platonic ideal of a strife-free Republic; the myth of an idyllic state of nature; utopian visions of co-operative communities; the Marxian prophecy of a society without coercion—all attest to a deep repudiation of violence as a form of behavior. Yet a major theme throughout this Report is that violence has been endemic in human history: the past is ridden with violence; we are surrounded by violence in the present; and the future holds promise of even greater violence. As Charles Tilly puts it: "The nature of violence and the nature of society are intimately related. Collective violence is normal."

We also view violence through a selective historical memory which distorts our understanding. There is a widespread tendency to deny the occurrence of violence, to suppress our knowledge of it. The past appears more peaceful than it was and present acts of violence seem ever surprising. To quote Tilly again: "The collective memory machine has a tremendous capacity for destruction of the facts." No less than moral repudiation of violence this distorted perception impedes attempts to study and explain violence.

Granted that resort to violence is a central fact of human history, what have been its overall effects or consequences? Several authors struggle valiantly with this question. They conclude that violence can have both good and bad effects; its consequences have been both positive and negative. In the words of historian Richard Maxwell Brown: negative violence is "in no direct way connected with any socially or historically constructive development," while positive violence is associated with "popular and constructive movement." But how can we tell positive violence from negative? Unfortunately, there is no clear basis upon which to distinguish the positive effects of violence from the negative. Brown himself includes urban riots as negative violence and agrarian uprisings as positive: surely too much depends on how one understands and appraises the particular causes at issue and the outcomes. This difficulty in evaluating types of violence confounds all who try to make such judgments. It seems easy to concede that violence can have positive effects —after all, the nation itself was born in revolution—but it is impossible to find a basis for evaluating the acts of violence around us.

Although the authors of the Report agree that resort to violence is a pervasive fact of social life, they show far less agreement on how violence should be studied. Differences in analytical approach are not simply a reflection of different disciplines: the approaches are more varied than disciplinary divisions would suggest. Even definitions of the subject vary. Some authors consider in their analysis only actions that inflict death or injury, while others include acts of "turmoil," such as strikes, demonstrations, and other peaceful collective behavior. Beyond these definitional differences, the studies also differ in other basic ways: in the levels of generality at which they approach the subject; the types of variables they stress as critical; the time-spans they adopt; and the kinds of evidence upon which they rely.

Levels of generality range from a medical psychiatrist's universal propositions about human nature to historians' accounts of episodes on the American frontier. The most convincing studies approach violence as a social condition which can be explained in general terms but which is at the same time related to particular circumstances. Two especially successful examples of this mingling of general and particular factors are sociologist Charles Tilly's "Collective Violence in European Perspective" and historian Sheldon Hackney's "Southern Violence." Tilly shows how different forms of violence accompany transitional and modern social and political conditions in Europe. Hackney attempts to explain the exceptionally high levels of violence in the South with several socio-economic variables, and concludes that psycho-cultural factors peculiar to that region must be taken into account.

A large section of the Report is devoted to two studies, one by Ted Gurr and the other by Ivo Feierabend, Rosalind Feierabend and Betty Nesvold. Both chapters are based on cross-national aggregate data analysis of the recent past. Gurr's study offers a general description of violence in the world from 1961 to 1965 and then turns to correlational analysis of the conditions of violence. Gurr faces serious difficulty in converting available quantitative data into convincing indicators of the theoretical variables he hopes to relate to violence.

One unavoidable conclusion is that we must have better measures of numerous independent variables before we begin accepting or rejecting hypotheses. As in his earlier work, Gurr's handling of quantitative material and analysis is sophisticated and insightful.

Feierabend, Feierabend and Nesvold face all Gurr's data problems plus those associated with a longer time period, 1948 to 1965, and overtime analysis. They recognize that most of the interesting hypotheses about violence are dynamic and require overtime data for testing or description. Although change variables are utilized, the analytic framework remains cross-sectional rather than longitudinal. It is clear that more refined data for longer periods of time are needed to do justice to the middle range theorizing we already have. Without such data it is hard to see how present theory about violence will be improved.

Other political scientists who contributed to the Report are Raymond Tanter, James Davies, and Edward Gude. Tanter investigates the relationship between escalation in Vietnam and forms of domestic violence and turmoil in the United States. He concludes that no relationship between the two conditions can be found for the period 1965-68. Davies attempts to account for instances of rebellion and revolution in terms of the frustration that accompanies rising expectations and decreasing gratifications. Gude examines factors associated with violence under Batista and Betancourt.

The Report is intended as background analysis for recommendations on national policies to prevent violence. Ordinarily we would not ask with respect to a collection of scholarly papers, Are they useful to policy-makers and advisors? But in this case the question seems more appropriate, even though the authors appear not to have become particularly concerned about the policy relevance and implications of their analysis. What can a policy-advisor draw on from the Report?

The policy-advisor can stress, first, that violence is a common condition in all types of societies, including modern industrialized societies. Second, although the incidence and forms of violence are related to general social conditions, distinctive factors in American history and society make us as a nation especially prone to individual and collective violence. Third, the effects of violence are not always negative, but no clear basis exists for determining what the effects of given acts of violence will be. Fourth, no widely-accepted explanation for the causes and patterns of violence is yet available, although several studies in the Report indicate that progress along these lines is possible. So far as the United States is concerned, the Report supports these propositions: given the social, economic and political conditions in the United States severe violence such as revolution and internal war is extremely unlikely; moreover, given these same conditions, even the relatively mild riots and demonstrations of the 1960's must be regarded as unexpected. Actually, much of the Report is deviant case analysis of regions of the United States and of particular forms of violence. All of which adds up, no doubt, to a plea for more research—a comforting conclusion perhaps for the scholar but less satisfactory for the policy-maker.

WILLIAM FLANIGAN AND EDWIN FOGELMAN
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*Puerto Rico: Freedom and Power in the Caribbean.* BY GORDON K. LEWIS. (London: Merlin Press, 1964. Pp. 622. $10.00.)

*Modernization of Puerto Rico: A Political Study of Changing Values and Institutions.* BY HENRY WELLS. (Cambridge: Harvard University Press, 1969. Pp. 440. $9.95.)

Puerto Rico has quite correctly attracted the attention of these two sensitive and erudite scholars to explain its political peculiarities. The Puerto Rican experience has special relevance for the Caribbean in particular, and for students of Comparative Politics generally.

All aspects of the island's political life are usually considered as unique or peculiar: its constitutional status, its culture, and the pattern of political leadership and organisation. These apparent peculiarities have not surprisingly led to two separate and widely differing interpretations in the volumes under review. Henry Wells presents us with a case study which is sympathetic to the efforts of the United States to transform a turgid Spanish-American culture into a dynamic Anglo-Saxon polity. Gordon Lewis, by contrast, is wary of this Americanization and explains Puerto Rico's difficulties as being directly a by-product of American control.

For Caribbean scholars, the Puerto Rican phenomenon has always proved somewhat puzzling, and these two case studies will go a long way in providing some of the answers. To begin with, they tell us a great deal about the efforts of the Puerto Ricans to transform their society. However, for those Caribbean states which have glibly assumed that the Puerto Rican experience can be easily emulated, these studies should raise some serious doubts about the desirability or the possibility of adopting the Puerto Rican model. Lewis and others have pointed out clearly some of the political consequences of following the pattern of what New World economists like Lloyd Best have called "industrialization by invitation." These studies indicate that this pattern may not necessarily be a solution for the twin problems of economic development and political stability.

Henry Wells, following the recent literature on political modernization, has attempted to look at the Puerto Rican experience as a case study in modernization. Some of us, and particularly those of us living in Third World countries, have raised some doubts about the use of this category, especially the normative content implicit in the doctrine as used by the apostles of modernization. Inkeles and Weiner in particular have not convinced us that modernization is not just another word for Americanization. With Gordon Lewis, we must query whether these analytical categories, especially the distinction between traditional and modern societies is indeed valid.

Apart from considerations of the theoretical validity of this concept in general, Puerto Rico, in common with other Caribbean societies, does not possess the characteristics which we normally associate with the traditionalist societies of Asia and Africa. Morse in another context has raised the whole question of whether New World societies do not present us with a special case that requires new analytical categories. Those of us in the area, especially the New World economists, have found it much more fruitful to use such concepts as the plantation system as the point of departure rather than the categories of traditional or transitional systems. Wells, unfortunately, has applied somewhat naively the notions of traditional society from the students of Afro-Asian politics, and has not taken account of the very special conditions that have created the Latin-American and Caribbean plantation societies.

Silvert, along with Morse, have long warned us of the dangers of including Latin America in the body of theory that has been built up about underdeveloped areas. It may be that Wells would have done better if he had left the theoretical baggage of structure-functional theory at home before he looked at the political experience of Puerto Rico.

One of the major dilemmas that both Lewis and Wells attempt to come to terms with is what the effects of this experiment have had on the Puerto Rican male personality. Wells is optimistic about the stability of the Puerto Rican personality, concluding that there has been a transformation from a submissive personality to a more rational one. Lewis, on the other hand, sees the continuance of docility and submissiveness, which is a cover for rage and anger. Elsewhere I have commented on the nature of the colonial personality, especially in the Commonwealth Caribbean. Superficially, we appear to have a special problem in that the Puerto Rican is neither a colonial nor a free man. He seems to live in a perpetual world of ambivalence, which places him in a special love/hate relationship with the United States. However, if Gordon Lewis is correct in his assessment of the Puerto Rican personality structure, it is very close indeed to that of the formally colonized individual which is also characterized by a high degree of authoritarianism.

It was Munoz Marin who fully understood this phenomenon. This remarkable political personality still awaits a good biographer. He is within the tradition of Caribbean political leaders who fully appreciate the function of a colonial leader as a bargainer or rather as a middle man between his masters and his people. Robert Anderson in his book "Party Politics in Puerto Rico" has already described for us in full detail his remarkable capacity to manipulate the electoral system and thus maintain political control for a decade. Anderson demonstrated how Munoz institutionalized his power base, transformed his party organization and related his party to government; thus offering us a structural explanation for the rise and the continued existence of the whole Munoz regime. He avoided the easy psychological explanations offered by Wells and at the same time also avoided Gordon Lewis' aversion to everything American.

The usual question that is asked by those who study politics dominated by a single personality is "after Munoz, what or who"? Munoz was certainly unsuccessful in trying to create a personality to take over where he left off. However, the real question is not "after Munoz, what or who"?, but rather the capacity of a system to decrease its dependence on a single leader and simultaneously the capacity of a party system to adapt itself to the new situation. The capacity for a political system to guarantee a successful transfer of power is what is normally associated with the term stability. Since Anderson examined the question in its structural setting he has come closer than the others in predicting the outcome of the most recent developments in Puerto Rican politics.

Puerto Rico may well be entering a new stage in its politics that may prove to be quite similar to what has taken place or is beginning to emerge in other parts of the Caribbean. One detects the emergence of a new kind of politics in the area where personalism may be giving way to the political managers who are capable of controlling the electoral machine either through gerrymandering and/or creating a party oligarchy. Concomitantly, however, when machine politics of this variety develops, and at the same time the basic economic problems of the society do not improve for large numbers of the populace, there is growing disenchantment of the youth and the unemployed with bureaucratic politics, and a growing cynicism towards the political system. However, as the political system has become more entrenched the elites become increasingly tense about the maintenance of the system and thus respond in a repressive way.

There appears to be a growing feeling in the region that political independence has not secured psychological decolonization and that this will not be achieved until there is economic independence and structural reform of the economic system.

These two recent works have contributed considerably to our understanding of some of the Puerto Rican "peculiarities." The time may be soon approaching when a more systematic comparative study of Caribbean politics will be forthcoming. This prospect appears more assured now that we have a number of case studies appearing with some regularity. It is only comparison with other Caribbean states with similar economies and social structures that will tell us whether the Puerto Rican experience has been unique. I don't think so.

A. W. Singham

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*The Study of Policy Formation.* Edited by Raymond A. Bauer and Kenneth J. Gergen. (New York: The Free Press, 1968. Pp. 392. $9.95.)

This is a landmark book in the study of public policymaking in the United States. Bringing together nine papers on the study of policymaking, the book surveys available knowledge, presents normative models, explores research methods, poses missions for the future, and provides concrete policy cases.

In the first chapter, the senior editor presents a general framework for the study of policymaking, and for the book. "Policy" being defined as "strategic moves that direct an organization's critical resources toward perceived opportunities in a changing environment" (p. 2), the study of policymaking should be concerned simultaneously with: (1) intellectual "decision-making" activities; (2) social-organizational policy-implementation processes; and (3) the dynamics of feedback and environmental change leading to policy revisions. Recognizing the limitations of available behavioral and normative models, Bauer proposes to study policy formation by fusing a social process view with a decisionmaking orientation, and puts forth a multidimensional research strategy, moving from systems survey and leverage points mapping to intense study of individual senior decisionmaking.

The next three chapters are devoted to surveys of available knowledge. Richard Zeckhauser and Elmer Shaefer provide a concise, inclusive and readable discussion of normative economic theory applications to public policy. The survey is innovative, for instance, in explicating its value assumptions, in discussing the costs of information and of flexibility, and in dealing with redistribution benefits in addition to efficiency benefits. Joseph L. Bower presents an excellent discussion of organizational and group decision theory, with an original emphasis on the ability to improve performance rather than achieve a rigorous behavioral theory. Enid Curtis Bok Schoettle (the only political scientist among the ten authors) concludes the state-of-the-art surveys with an analysis of political science contributions, which is somewhat outdated—probably because of technical reasons.

The next two chapters by Kenneth J. Gergen deal with policymaking study methodologies. First, Gergen provides a three-dimensional model for the identification of leverage points, to be mapped by issue relevance, personal efficiency, and policy sub-phase. With the help of a variety of survey techniques, individuals are to be classified in terms of this model—individuals being regarded by Gergen (who is a psychologist) as the most important sub-unit of the policymaking system. The second methodological chapter provides an elementary discussion of basic research methods, which is perhaps out of place in the book as a whole, but which includes some important observations on the highly neglected study of individual high-leverage decisionmakers.

The case studies concluding the book deal with urban mass transportation (by Lewis M. Schneider), with technology transfer from NASA to the civil economy (by Edward E. Furash) and with decisionmaking on foreign aid (by Theodore Geiger and Roger D. Hansen). Quite interesting by themselves and rounding out the book as a reflection of the contemporary study of policymaking, there is nevertheless little internal relation between these descriptive chapters and the rest of the book: The theoretic concepts and methodological recommendations of the first six chapters are not really utilized in the case studies, and the data presented in the last chapters is not relied upon in the first part of the book. This is a weakness; but I think this weakness serves to illuminate the state of study of policymaking till very recently.

I started this review by calling this book a landmark. It is indeed a landmark in presenting most of what is known now and in indicating some of the needs for the future. Most of the book's weaknesses are faults of the contemporary study of policymaking—which cannot but break through despite the efforts of the editors. Two such main weaknesses are the tendency towards micro research and reductionism; and ambivalence on the relations between behavioral and normative approaches.

(1) Most parts of the book follow micro and reductionistic approaches to the study of policymaking, in which main emphasis is put on instances of incremental policymaking within a slowly changing situation. Social movements, aggressive ideologies, radical changes in conditions, survival issues, needs for far-going innovations—all these phenomena, which by now are no longer strangers to the United States, cannot be perceived, analyzed and dealt with by the frameworks, models and methods available in the present study of policymaking as reflected in the book. Especially surprising in 1968, and nevertheless typical, is the statement by Schoettle that "dramatic breaks with the past such as the atomic bomb, or the space program . . . can be treated as *random disturbances* in the otherwise stable and incremental policy-making process, only *momentarily* producing a fluctuation in the old patterns of policymaking which quickly reassert themselves" (p. 119, emphasis added). It is such jumps which shape the whole policy-space and should be among the foci for study and improvement. But in order to deal with such happenings, policymaking must be regarded as a complex systems phenomena, with emphasis on the study and evaluation of the policymaking system as a whole and development of models for the improvement of meta-policy (that is, policies on how to make policies), including policymaking system redesign. And such an approach needs more than empiric study of micro situations, incrementalism and economic model building.

(2) The strong desire of Bauer and some of the contributors to help in meeting the urgent needs of policymaking improvement are chained by the above mentioned limits of contemporary behavioral research methods on one hand and the narrow domain of available normative models on the other hand. Bauer, seeing this dilemma, expresses high hopes for long-range improvements of practice while recognizing that all that the practitioners at best can get out from available knowledge "is . . . to deepen his comprehension of the range of problems with which he is accustomed to deal and thereby to help him to invent better solutions of his own" (p. 5). How to change our behavioral study of policymaking and how to develop new normative models for improving policymaking—these are questions posed by Bauer but not taken up in the other chapters of the book. Indeed, even some of the already available more relevant ideas—such as policy impact evaluation, explicit post factum and real time social experimentation, sequential decision models, alternatives futures construction and strategic analysis methods—are not mentioned in the book, neither as theoretic frameworks for innovative behavioral research nor as approaches to policymaking and metapolicymaking improvement.

Even though there is only one political scientist among the ten authors (or, a cynic may say, because of that fact), this book should be carefully studied by the increasing number of political scientists worrying about how little our discipline has to contribute to better policymaking. Learning from the richness of what is included and its inadequacies, we still stand a chance of building up policy sciences—if we are ready to make the necessary jumps in our own concepts, methods and traditions. This book indicates that if political scientists lag behind, there is reason to hope that the necessary job may nevertheless be done by others.

YEHEZKEL DROR*
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*The Politics of the Southern Negro: From Exclusion to Big City Organization.* BY HARRY HOLLOWAY. (New York: Random House, 1969. Pp. 374. $8.95.)

Professor Holloway departs from the tradition of V. O. Key, Jr. and Prothro and Matthews in this study of Southern politics. The *Politics of the Southern Negro* is a straightforward narrative account of the politics of the region, and includes not a single chart or table. And though the book is replete with insightful observations about politics in the South, its methodological shortcomings, lack of an explicit comparative framework, and inadequate conception of political change, greatly restrict its value for the serious student of the Negro and politics in the South.

The author is well aware of the limitations of his work. At the outset, for example, he concedes that his method of gathering data for the study was inadequate. The primary source of data for the *Politics of the Southern Negro* was interviews which the author held with 200 Southerners, black and white, in the communities he studied. As a Northern white, in an alien setting, he found that his "main problems were interview bias and inaccurate or hazy factual information." We should not exaggerate our eyebrow raising about Holloway's procedure, however; for we recall that, in 1964, when much of the data for the book was gathered, even the Survey Research Center at Michigan had only recently become aware of the dubious validity of white-to-black interviews.

The essential problem that Holloway is dealing with in this study is how to account for political change and development among Negroes in the South, though he does not state the problem this way. His approach to the problem is to conceptualize the South as a "modernizing traditional society," and, within this framework, to analyze the political behavior of blacks on a continuum from the extremely traditional society (e.g., Mississippi) to the modern society (e.g., Atlanta). At the same time, he attempts to apply the ethnic group formulations of Lane and Dahl to the political behavior of blacks in these communities. (His conclusion about the applicability of these formulations to the black experience is not unambiguous. Nevertheless, he does seem to feel that the black situation cannot be analyzed in traditional ethnic group terms because "no other group has come from such low status and had to battle so hard to win even a modicum of the rights taken for granted by other Americans.")

Professor Holloway offers us no explanation as to why he chose to investigate the eight communities he did, although the study for the most part vindicates his intuitive judgment as to what is "traditional" and what is "modern." Another methodological difficulty presented by the study is the fact that the political units of analysis are not comparable. In Mississippi he deals with the state; in Macon County, Alabama, Fayette County, Tennessee, and Marion County, Texas, he deals with counties; and in Birmingham, Houston, Atlanta, and Memphis, he deals with cities. There may have been sound reasons for this, but Professor Holloway does not set them forth in the book.

But the most serious shortcoming of the study, from my point of view, is the absence of a genuine comparative framework to guide the research and the analysis. The study would be much more useful had the author taken systematic account of the effects of (correlation between) such variables as: the formal rules and structures of each of the systems; their environmental characteristics; the political cultures and decision-making structures; the role of personality; and the impact of the non-systemic forces (e.g., the Federal Government) on black political behavior in the eight communities. This is not to say that he does not deal with these factors, but to say only that they are not built into his analytical framework. As a consequence, it is almost impossible to know what the major dynamics of change are, and why communities next to each other on the traditional-to-modern continuum differ so much. Having said this, however, I must add that the Federal Government emerges from this study as one of the major modernizing forces in the South.

Professor Holloway found that black politics differs from community to community, even within the so-called modern or developed systems. Blacks in Houston tended to team up with the Latins, labor, and liberal white Democrats. The author does not feel this coalition is necessarily to the advantage of the blacks. In Atlanta, they allied themselves with the white patricians. Holloway does not find anything wrong with this coalition, but he is skeptical about whether it is the best of all possible arrangements for blacks. (He correctly predicts the breakdown of the coalition.) His ideal seems to be Memphis where, since Crump, blacks have developed a hard-nosed, non-ideological attitude toward political alliances, and have tended to enter them on the basis of pragmatic considerations from election to election. Memphis seems to be an interesting city, insofar as the political development of blacks is concerned; and it would repay much more careful and systematic investigation. Holloway's work suggests a curvilinear relationship between education, income, and political sophistication as between the better educated and higher income population of blacks in Atlanta and Houston, and the lesser educated, lower income blacks population of Memphis. It is astounding that the number of blacks registered to vote in Memphis rose from 7,000 in 1951 to 93,000 in 1964! This is 75% of the black population. (By comparison, for example, there are only 80,000 blacks registered in Atlanta as of September, 1969—and the black population there exceeds that of Memphis by about 50,000. Percentage-wise, black registered voters in Memphis outnumber those in Atlanta by almost a 2-1 margin.)

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* Any views expressed in this paper are those of the author. They should not be interpreted as reflecting the views of The RAND Corporation or the official opinion or policy of any of its governmental or private research sponsors.

In my opinion, one of the strong points about Professor Holloway's study is the fact that he makes his value judgments openly rather than covertly. After reading the ubiquitous assessments of the black experience by such political scientists as those at the Harvard-MIT Joint Center, it is a pleasure to find at least one white political scientist concluding that "The black American need not defer to the accomplishment of any other Americans, whatever their origins and talents."

Tobe Johnson

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*Political Leadership in India: An Analysis of Elite Attitudes*. By Michael Brecher. (New York: Frederick Praeger, in cooperation with the Centre for Developing-Area Studies, McGill University, 1969. Pp. 193. $12.50.)

Of the non-Western areas, India is certainly one of the most written about and studied by political scientists. Although there have been several background type analyses of various Indian elites, there has not been a serious examination of how the Indian national elite perceives its role and how it interprets the political system in which it operates. Michael Brecher's *Political Leadership in India* meets this need. The book does not, and it should be pointed out that this was not the author's principal goal, tell us very much about the Indian elite structure in general nor does it add very much, except as a source book, to our understanding of comparative elites.

In March of 1967 Brecher sought the cooperation of eighty-two Indian political leaders in New Delhi for the purpose of administering a questionnaire. Eighty agreed. This group consisted of 63 politicians, 6 academics, and 11 journalists. All the respondents, excepting two of the journalists, were Indian. The questionnaire, which Brecher includes in an appendix, was administered in all but one instance by the author and most interviews lasted a little more than an hour. With only a few exceptions, the author reported good rapport and cooperation from the interviewers. The questions were highly specific and were designed to elicit replies in three areas: the reasons for the Congress decline in the 1967 elections, the qualities of leadership and the ranking of Congress leaders, and the contest for the Prime Ministership in 1967.

The resulting work makes a major contribution to our understanding of contemporary Indian elite perceptions in these three areas. Each of the 32 questions is analyzed separately, with a nearly complete breakdown of the responses by elite category. Perhaps the most interesting finding concerning the elite's perception of the Congress reverses is the strength of the focus on political institutions rather than on public policy. About forty percent of the Congress M.P.s perceived no connection between any government policy and the Congress reverses. Of the remainder, devaluation was mentioned as a contributing factor by about 53 percent of the congress M.P.s. The other elite groups shared this same emphasis. Institutional factors such as Congress factionalism and a more united opposition tended to dominate the interpretations of the results. Brecher suggests that, in Almond and Verba's terms, the elite perceives the mass as a "subject" clientele. We should not fall into the fallacy of misplaced significance here: perhaps elites everywhere give a similar weight to organizational over substantive factors. In any event, the unresponsiveness that this attitude suggests would appear to be unfortunate in the Indian context.

The same theme reappears in the discussion of qualities of leadership. The emphasis here among the Congress elite was on qualities associated with maintaining organizational effectiveness, not on policy effectiveness. Thus, Morarji Desai was the Congress elite's second choice, despite the fact that he ranked highest in qualities associated with making successful economic policy. He was, in the Congress elite's eyes, inferior to Mrs. Gandhi in organizational skills, and so was less attractive, over all, as a Prime Minister.

Brecher has as keen an ear for what is not said as for what is. He notes in this chapter that not one member of the elite, official or non-official, Congress or non-Congress, considered skill in foreign policy to be necessary to a Prime Minister. Another notable "non-effect" was that Mrs. Gandhi's charismatic link to her father, in the opinion of most respondents, was of no or only marginal utility to the Congress.

Of course, Brecher says a great deal more, and briefly relates his findings, at the end of the book, to Apter's reconciliation/mobilization typology. The only major methodological addition this reviewer would have liked to have seen would have been the computation of the fourfold *phi* at several places. Hopefully, the material upon which the analysis rests will be made machine-readable and offered to the scholarly community.

The disappointing aspect of the book is the author's substantial failure to relate his findings to the large body of elite studies that have been made. Although there have been some notable exceptions such as Russett's *World Handbook* and Almond and Verba's *Civic Culture,* progress in comparative politics has not been realized through studies with an explicitly comparative focus, but rather the same topic has been approached in different ways in different countries in different time periods. Eventually a body of work develops, and, despite a great many minor irregularities, common themes become evident. A notable example of this in the field of elite studies, as Samuel Patterson has pointed out, is the trend toward occupational democratization in the membership of parliaments as a function of time.

This method of building a science of comparative politics may be awkward and may often slip into trivia and bad political history, but given the country-focused interest of most comparativists and the organization of the academic environment, it is likely to remain the way it must be done for some time. It is therefore incumbent upon a scholar, when he has written a work dealing with some topic in his country of interest, to relate his findings systematically with the body of work in other polities. Comparative political studies has, like art, illuminated its subject matter by presenting the universal in terms of the particular. Consequently, an author should not let the more general (and thus analytically more important) significance of his work remain obscure, hoping that some other scholar will relate it to the sub-discipline as a whole.

In this regard, Brecher states that "There is no body of literature which deals systematically with elite images of one's own political system." Although this reviewer can not recall another work that follows Brecher's closely, there are certainly a substantial number that are relevant to it. These would certainly include, for example, Deutsch *et al., France, Germany and the Western Alliance: A Study of Elite Attitudes on European Integration and World Politics,* Seligman's *Leadership in a New Nation,* and, taking a different approach, Cnudde and McCrone's "The Linkage beween Constituency Attitudes and Congressional Voting Behavior." There is also a major body of work dealing with Indian politics *per se* that would have illuminated *Political Leadership in India.*

Unfortunately, Brecher makes very little reference to this literature. Only five scholars are cited in the entire work, and they very briefly. One citation of W. H. Morris-Jones simply identifies him as the Director of the Institute of Commonwealth Studies, but does not refer to the relevant work. On the same page, a reference to the "consequences of the election results for India's political system" is footnoted as "By various American and Indian scholars." A plurality of the citations in the volume, which number fewer than twenty, cite the author himself. There is no bibliography.

Even given the difference in approach toward explicitly relating ones work to the work of others in the British and the American traditions in scholarship, Brecher does very little in this regard. However, it should be remembered that this is a criticism of what Brecher did not do. What he did do he did quite well, and it is much needed.

ROBERT SIDWAR ROBINS

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*Bureaucracy and Participation: Political Cultures in Four Wisconsin Cities.* BY ROBERT R. ALFORD. (Chicago: Rand McNally & Co., 1969. Pp. 224. $6.50.)

Since the time in the late 1950's and early 1960's when Edward C. Banfield, Robert A. Dahl, and others restored by their work intellectual vitality to the study of urban politics, political scientists have been groping for new directions to take in a field that has now, with the student and popular interest in cities, become a matter of general concern. The methodological debate over how to study the distribution of power is not settled, but increasingly it seems sterile and, one suspects, perhaps irrelevant. How power is in fact distributed is hardly irrelevant, but finding a theoretical approach by which to assess the consequences of that distribution seems more important.

One possibility is to study the policy outcomes of local political arrangements, but measuring those outcomes for any large number of cities has not proved as easy as some at first hoped. Another is to conceive of American urban politics in terms of the concepts of political modernization. Doing this in a way that produces more than merely the substitution of one set of labels for another ("modern" or "mobilized" in lieu of "professionalized council-manager systems" and "traditional" in lieu of "political machine systems") is harder than it first appears; in addition, concepts that seem to work for backward nations about which we know relatively little seem inadequate for cities about which we are a good deal better informed. A third approach might be to look at the relationship between the enduring features of a city—its economic base and its population characteristics—and its political behavior, indicating thereby the constraints within which power can be exercised and perhaps also the sources of change in the distribution of that power, but even if successful such a view does not make any easier answering the question of how political arrangements intervene between socio-economic factors and public policy.

Robert R. Alford, a sociologist at the University of Wisconsin, has bravely tried, in a slim but rich book, to explore all three conceptual positions, linking them into a more or less coherent theoretical structure and illuminating that structure with data drawn from four Wisconsin cities—Madison, Racine, Kenosha, and Green Bay. He begins with the differences in the economic bases of the communities as a way of understanding why each has a different political culture, defined in terms of two variables: the degree of bureaucracy and the level of participation. The highly bureaucratized, participatory system he terms "modern," the unbureaucratized, non-participatory system he calls "traditional." A trading center (Green Bay) is more likely to have a traditional political culture, a professional center (Madison) is more likely to have a modern culture, and cities with a manufacturing base are likely to be mixed cases depending on whether the economic leadership of the community is inactive and drawn from absentee-owned large corporations (as in Kenosha) or energetic and civic-minded because of the special character of locally-owned firms (as in Racine).

Each political culture has a set of distinctive "public norms": in a modern city, the police are highly professionalized (and thus issue a lot of traffic tickets and arrest a lot of juveniles), the courts are busy with a great deal of litigation, the political system provides many opportunities for women, and the zoning officials tend to apply their ordinances strictly and consistently. In the traditional city, the police are less zealous, the courts less busy, the female politicians notable for their absence, and the zoning officials more inclined to accommodate special requests. These public norms—the universalistic ones of the modern city, the particularistic ones of the traditional one—are in turn productive of different kinds of public decisions. Madison has several heavily-funded poverty programs; Green Bay has only one program with token funding. Madison was a leader in getting an urban renewal program underway, Green Bay was much slower and the other two cities have yet to start. And of course Madison was among the first cities in the state to fluoridate its water supply.

These "political cultures," it turns out, are properties of the structure, functioning, and history of local government, *not,* so far as Alford could tell, of the attributes of citizens. He found no distinctive orientation or ethos among voters that was associated with residence in one or the other city; religion and social class explain, in the usual manner, the attitudes of the citizens. In the modern city, college-educated persons get to be leaders who, whether liberals or conservatives, tend to be cosmopolitans; in the traditional city, less-well-educated persons occupy leadership positions and take a more parochial or localistic view of matters. In short, differences in attitudes are real but they seem not to be the product or a predictor of local political arrangements; on the other hand, differences in political structures tend to persist in part because they recruit different kinds of persons into leadership positions.

Obviously, this is a stimulating and in many places quite original work. It is a major effort to provide a new and coherent intellectual direction to the study of urban politics, and thus deserves wide attention. But it also raises as many questions as it settles. The measures of bureaucracy and participation leave a good deal to be desired: it is hard to distinguish "bureaucracy" (the multiplication of offices and specialization of function) from "professionalization" (the emergence of administrators with "professional" norms and reference groups). Thus, Madison's innovativeness may be less a function of bureaucracy than of a special kind of bureaucracy—one with strong professional orientations. Similarly with "participation"—Alford's data refers primarily to *organized* forms of participation (active political parties, intensive campaigns many civic groups, high voting turnout) and thus he concludes that Green Bay is not "participatory." Yet in the account of Green Bay politics, we are told that almost all Green Bay leaders went "to great lengths to insure consensus before they acted" and the mayor in particular "makes concessions, negotiates, and feels out the ground before making a move." (p. 49). I would bet that one reason why so little happens in Green Bay is precisely because it *is* so participatory in an informal way—if anybody has any strong objections to anything, the proposal is blocked.

Given these conceptual problems, and noting that the mixed cases (Kenosha and Racine) never turn out to be very interesting, I would suggest that a simpler way of defining these political cultures that is consistent at least with the Wisconsin data is the *level of organization* of public life. Madison is highly organized publicly (in bureaucrats and parties) and privately (in civic groups); Green Bay is less organized. To go beyond the Wisconsin cases, I suspect one would have to distinguish among *kinds* or *modes* of organization, whether particularistic and person-regarding (as with a political machine or fraternal lodge) or universalistic and community-regarding (as with a Good Citizens' League or Housing and Planning Council). But this is all speculation, not criticism, and indicates that Professor Alford has opened up some fresh perspectives and provided some new directions in the study of urban politics.

JAMES Q. WILSON
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*Peace Among The Willows: The Political Philosophy of Francis Bacon.* BY HOWARD B. WHITE. (The Hague: Martinus Nijhoff, 1968. Pp. 266. Guilders 33.)

Professor White has brought to fruition his vast knowledge of the political writings of Francis Bacon in a work that is at once scholarly, systematic, and engaging. The author's sometimes astonishing interpretive and synthesizing abilities combine to make this perhaps the most novel and thorough account of Bacon's scattered thoughts on politics, morality, and science. At a time when political philosophy is struggling to gain its bearings and relate itself to the social science enterprise, it is helpful to reconsider Bacon's contribution in the context of White's very divergent and perhaps equally unsatisfactory position.

White's identification and discussion of Bacon's two societies, his utopia where science rules, and the society which was a condition necessary for constructing the former, leads him to divide his attention between Bacon's provisional morality and politics and his definitive morality and politics. The account of Bacon's provisional morality and politics is gleaned and constructed from a great range of disparate sources. White argues that Bacon attempted to reconcile scientific advance with political conservatism and tried to overcome the obstacles of despair by preparing men's minds to hope. In discrediting the old ways symbolized by Aristotle and superstition, Bacon hopes to inculcate the virtues of charity, political pliance, and a high regard for external goodness and fortune. Bacon prefers a monarchial government of cautious imperialistic dispositions to stimulate science, civic peace, commerce, and religious toleration. White believes that Bacon saw Britain as the ship that would take mankind to the New Atlantis.

Bacon's utopia, the New Atlantis, is, like Bacon's natural philosophy, still futuristic and not yet complete. It is, however, a place where science conquers chance and determines change, where royal power diminishes, commerce withers, and the rule of humane and compassionate scientists prevails over statemanship and war. This ancient christian society glories in the beneficence of scientific invention guided and controlled by philosophy. Rational hedonism characterized by sober luxury, beauty, health and longevity is made everlastingly pleasant and stable by the reconciliation between patriarchical society and a young, vigorous, and progressive science.

White's purpose is not only to explicate Bacon's political thought but to expose the sterility and danger of political faith, the distinctive characteristic of utopian thought. It is said that the prevailing faith in a beneficent science, in experts, and in the ultimate solution of political problems is a debt that we uncomfortably owe to Bacon. Now White has many useful comments on the question of utopian thought however questionable it is to discard Plato and More as utopians because to them nature is something that is fixed. (Although sometimes they are said to be simply pre-modern utopians). It is disconcerting to discover that the remedy advanced by White to combat an unregulated and dangerous utopian faith in science is nothing less than a political philosophy with an element of faith in no smaller proportions or pervasiveness. The only hope is for a political philosophy which can know the good, what man is, what the best political community looks like, the nature of the soul and even the cosmos. White surely equals Bacon in the scope and unconstructed character of that which we are asked to believe. There is little justification for this new faith. Few reasons are offered to show how it is less blind, less dangerous, and more qualified to guide practical decisions and to ordain the proper paths of scientific invention and use. It is in confronting science with his view of political philosophy that White is most disappointing. There is little here that is either novel or penetrating. The old Straussian denunciation of science as ill-suited to determine values, or judge what is a healthy society or what is just provides a less than compelling or systematic treatment of the question. Nor does White entertain the possibility that the fruits of science, especially a thoughtful social science, might be of some assistance in the development of a political philosophy.

White's mode of analysis is no less Straussian and no less demanding of faith, however insightful and productive it appears to be. White does not suppose that a great writer "would make contradictions, flagrant contradictions, absent mindedly. . . ." (12) and so by ingenious interpretation of Bacon's use of words, fables, myths, numbers and colours, he exposes the "secret teaching" on a subject matter which is at once "hard to know" and "unfit to utter," a teaching addressed to the "legitimate and best minds" (110). White's impressive scholarly interpretation loses some of its force insofar as the code is never clearly explicated, the criteria in terms of which certain meanings are selected and evaluated is never precisely outlined. This procedure may be inescapable in making sense out of Bacon, but the result is clearly White's Bacon. Consider the ingenious detection of Bacon's meaning in his presentation of the chief scientist clad in peach coloured shoes, a white gown and a black cloak. We learn that "when built on eros or standing on eros, that which receives all (material cause) may be covered by that which receives nothing (final cause)." (196) This proceeds from White's use of ancient and contemporary sources, the ancient ones being those "with which Bacon *must* have been familiar and which *may* have had some meaning for him." (175)

White contends that modern man, whether liberal or socialist, is "essentially Baconian," that it was Bacon who stole fire from the gods and led us into the labyrinth of modernity. We are said to be bound by the world created by modern thought "particularly the thought of Francis Bacon." It is surely paradoxical that a secret teaching of such devious and hidden meaning, not accurately exposed before White's subtle examination, should have enjoyed such a direct and pervasive influence. Perhaps the factors that influence the course of events are more hidden than even Bacon or White imagined or possibly they are much more evident and straightforward.

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*Elections in America: Control and Influence in Democratic Politics*. By Gerald M. Pomper. (New York: Dodd, Mead, and Company, 1968. Pp. 297. $3.95.)

Pomper declares his purpose in his preface: "As a citizen, I have been disturbed by an apparent public cynicism about elections in the United States and the consequent disdain for the political process . . . This book is a deliberate effort to show that elections do have significance." (pp. *ix–x*) To accomplish such a purpose Pomper seeks to do three things: (1) to review, categorize, and synthesize the writings of political theorists concerning elections; (2) to briefly summarize and interpret the existing literature dealing with American political processes and institutions; and (3) to present the findings of four of his own research undertakings.

For his theoretical review Pomper fits together quotations from Plato, Aristotle, Machiavelli, Locke, Rousseau, Madison, Hamilton, Calhoun, and Mill; his own personal contribution is an attempted distinction between the direct and indirect effects of elections. "We will concentrate on two broad alternative effects—direct and indirect results of popular elections. Direct voter control would be evidenced by elections in which the choice of men also clearly indicated popular preferences on policy issues . . . In the absence of such mandates elections may have indirect significance. The actions of politicians might not be controlled in detail by ballots, but they might act in anticipation of voter reactions." (pp. 12–13) "Theorists generally have evaluated elections unfavorably when they have focused on elections as direct choices of wise men or wise policies. In contrast, they have been well-disposed to the democratic process when they have considered the indirect effects of popular choice." (pp. 32–33) "To provide criteria for evaluating the ballot, we have focused on elements of the 'traditional theory of democracy.' Our analysis suggests that this theory has been misunderstood and, consequently, that many modern criticisms of elections are misplaced . . . The advantages of elections have been seen in their indirect effects, particularly to protection of the voters, not in the wisdom of their decisions. To test the worth of elections, we should focus on their reputed benefits . . . It was Plato, after all, not Mill who measured the quality of a government by the wisdom of its rulers and the absolute truth of its decisions." (pp. 38–39)

The distinction between direct and indirect effects—which, once made, is used recurrently throughout the book—does not appear as useful to me as it does to the author. The difficulty is that both direct and indirect effects are premised upon the same ultimate claim: that voters are competent to differentiate candidates whose policies are favorable to their interests from those who are not. The only difference between the two cases in reality seems to lie in the actions of party managers and candidates rather than voters. In the one case by correctly foreseeing voter reactions and adjusting their policy proposals accordingly they guarantee that elections can have only indirect effects; in the other case the direct effect of the election upon policy is created by the actions of one or both party leaderships in misreading voter reactions and offering sharply divergent platforms. The underlying problem rests in Pomper's conception of linkage. He is presumably interested in the interrelationship between public preferences and public policies, but prejudges the question of where the linkage between the two must take place. In the preface he tells us that "Elections provide the linkage between the behavior of the voter and the actions of government . . ." (p. *ix*) and later: "In most castes, the voters can affect policy only through the election of politicians who will take actions desired by the majority of citizens." (p. 12) Most analyses of the linkage between public opinion and public policy avoid such narrow definitions of focus and the resulting confusion is evident in Pomper's quotations which intermix judgments concerning the wisdom of choosing policies on the basis of mass preferences with evaluations of the effects of elections in shaping policies.

The two descriptive chapters that follow manifest Pomper's awareness of other types of linkage and are among the strongest parts of the book. Chapter 4 on "Voters in America" is particularly outstanding and is the best succinct summary of the current knowledge regarding American voting behavior that has come to my attention. To those already familiar with this literature, however, Pomper's four attempts at addition will be of greater interest. The first of these—based in large part upon an article published in 1967 in the *Journal of Politics*—surveys party voting in presidential elections from 1828 to 1964 and seeks "to locate the historical occasions of party changes in presidential elections and to assess the policy effects of these changes." (p. 99) Fundamentally the essay is a classification of presidential elections by type, based upon Key's notion of the critical election as elaborated in the Survey Research Center's threefold definition of terms. In translating these concepts to match his electoral data, however, Pomper distorts them in two serious ways. In place of Key's conception of a critical election as defined by its persisting effects Pomper substitutes a notion of critical change as sharp deviation from the past. Thus 1960 and 1964 both become critical elections—in Pomper's terminology, "converting elections"—without any demonstration of their impact upon later electoral patterns. Pomper also replaces the original assumptions of continuity and change in the electorate with correlation coefficients for the state-by-state presidential vote. The convenience in terms of available data sources is obvious, but the result is to make it impossible to evaluate Pomper's rejection of the conventional view that 1936 was a critical election of major import since his statistical techniques prohibit him from capturing intra-state party changes defined in class terms. Most seriously in terms of the book's larger purpose these voting changes are related to governmental activity only through the citation of a few commonplace historical sources, and the reader is left in doubt whether the election returns made any difference or—as Pomper neatly puts it—"Social trends were preparing a new political order." (p. 113)

Pomper then turns to the comparative study of American State politics for evidence. His topic is an interesting one: the relationship between elections for governor and state fiscal policies. In other work Hinckley has demonstrated that incumbent governors are far more vulnerable electorally than incumbent congressmen or Senators (or Presidents) while Sharkansky has spread out in his *Spending in the American States* a table of changing expenditure levels by states that can be read as a roster of the strong governors produced by the 20th century. Pomper's contribution is to relate two variables measuring the electoral success of the governor's party to five measures of tax increases and five measures of spending increases. His general conclusion is that no significant relationships exist and that it is mistaken wisdom to assume that increasing taxes contribute to a governor's rejection by the voters although suggestive yet inconclusive variations appear between Democratic and Republican governors and more competitive and less competitive states. As Pomper rightly notes: "In a science of politics, disproving hypotheses is as important as their confirmation" (p. 129), but in terms of his overall purpose of demonstrating the significant impact of elections the findings must be counted as negative.

Evidence far more convincing to the case is provided by the third analysis, undoubtedly the most interesting section of the book. In two chapters—parts of which have previously appeared in the *Midwest Journal of Political Science*—Pomper exposes the Democratic and Republican party platforms of 1944, 1948, 1952, 1956, 1960, and 1964, to sentence-by-sentence content analysis and then compares the promises made with the promises kept. He demonstrates that campaign platforms are far less matters of empty rhetoric than often supposed, and that they consist in significant part of intelligible, specific pledges of future action. In all he finds 1,399 pledges which together constitute 27 per cent of all sentence statements in the 12 platforms. His central finding is that where both parties have made identical promises some 85% of the pledges have been substantially fulfilled; that the victorious party has carried out 79% of the promises idiosyncratic to its own platform; and that even the defeated party has succeeded in effectuating 53% of its pledges. As these figures suggest, Pomper also finds that the distinctive platform promises of the two parties are not usually in conflict, but reflect instead the differential campaign strategies of the two parties with the Republican platform emphasizing such nondistributive issues as defense and general government and the Democrats stressing particularistic gains for identifiable groups in matters of labor and welfare policy. For the limited range of issues on which the two party platforms do contain directly conflicting pledges Pomper provides overwhelming evidence of the effects of election outcomes in policy outputs, with 80% of the actions finally taken consistent with the promises of the winning party.

At minimum it can be said that Pomper has demonstrated that elections are occasions for policy-makers to summarize the policies they plan to pursue; at maximum it can be asserted that Pomper has proven that within certain areas of policy the victory of one party in the election has determined the programs that will be enacted. This by no means exhausts the content of the chapters, however, for the tables—though not strikingly easy to read—contain materials for many other conclusions, some but not all of which are discussed by Pomper. His detailed analysis of platform content is particularly instructive as to variations in campaign strategy between the parties, over time, and among policy areas, and his analysis of pledge fulfillment shows that promises are most often kept in respect to defense, agricultural, and resource policy, and least often in matters of civil rights and labor.

The subject of civil rights provides the material for Pomper's final analysis, the most controversial, difficult, but important test for his proposition. He seeks to prove that the protection of Negro rights derives at least in part from possession of the ballot. Using the standard sources he summarizes the history of Negro political activity during Reconstruction and after and concludes: "It is an indication of the importance of the vote alone that black interests continued to be considered even after white supremacists returned to power." (p. 213) Although he may well be right Pomper is in fact unable from his data to disentangle the effect of the possession of the ballot from other influences affecting the position of the Negro at the time. Pomper's personal addition to the existent literature on the period is a series of tabulations of lynchings, public school enrollments, etc., intended to demonstrate that the vote was a determining element. He argues, for example, that disfranchisement removed one of the restraints on lynching since "political power was no longer even a possible defense, and the increase in violence evidenced the change." (p. 217) The relationship must remain speculative, however, when so many other uncontrolled variables are blatantly at work. It is simply not true to say, as Pomper does, that: "We cannot prove causation, but an association of lynching and disfranchisement would indicate the influence of the ballot." (p. 217) If the correlation is, in fact, spurious, and both variables depend upon some further unspecified variable the term "influence" is clearly inapplicable. The tabulations for lynchings and capital punishment are still further flawed by the failure to convert them to the per capita terms that would make them meaningful. In sum it seems likely that Pomper's argument will not convince anyone who begins reading with the prior conviction that black advances have been the result of other forces than the vote. The paradox runs deep, of course: if Negroes are permitted the vote when they pose no threat and denied it when they do, does this prove that the vote is a powerful influence? or a powerful symbol?

One final comment should be made concerning style. In his preface the author warns: ". . . (A)s a teacher, I have been upset, and ultimately bored, by quarrels over methodology. Tenaciously, but intolerably, opposing dogmatists insist that meaningful research can only be conducted through their prized techniques. In this work, I have deliberately been eclectic in the use of methods." (p. *x*) His editor, David Fellman, agrees and tells us: ". . . (I)t is refreshing to read a competent book which cheerfully makes use of various methodologies without being enslaved to any one and without making a fuss about it." (p. *viii*) I must disagree. An author can be enslaved by eclecticism as much as by any other methodology. Like most political scientists Pomper does some things better than others and the effort to do a little of everything throws the book out of balance. Some chapters read like a well-done supplementary textbook; others like a well-done research monograph; others like a not-so-well-done research monograph; and one like an effort to include a chapter on theory to prove a point. At times eclecticism carries the author into subjects of which he knows too little. "(T)he British House of Lords agreed to its own loss of power after the voters supported the reformist Liberal party in 1910, and the U.S. Supreme Court ended its opposition to the New Deal after Franklin Roosevelt's landslide victory in 1936." (p. 66). And so on. The book would be the better for fewer "methodologies" and more concentration on the subjects that Pomper handles well and on which he has much to say.

FRANK MUNGER

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*The Soviet Prefects: The Local Party Organs in Industrial Decision Making.* BY JERRY F. HOUGH. (Cambridge: Harvard University Press, 1969. Pp. 416. $12.50.)

Only recently have books on aspects of the Soviet political system made use of theories and models from the social science disciplines for purposes of either explication or verification. Instead, the tendency has been to engage in a detailed presentation of data and information that the author thought pertinent to his subject matter. Although some very good studies have been written this way, the study of Soviet government has tended to constitute a distinct enclave within the discipline of political science. Of course, this phenomenon is and will continue to be unavoidable to some degree, if only because the study of Soviet politics obviously carries with it several difficulties not present in study of the United States polity.

One reason this book is to be noted favorably is that it utilizes theoretical constructs in the examination and elucidation of the Soviet political process being studied, namely the nature of the involvement of the organs of the Communist Party at the *oblast'* and comparable levels in industrial management, planning, and decision-making in general. This is combined with the lucid presentation of the results of detailed research which clarifies and increases our knowledge of the people in the local organs of the Party, the work they do, and the ways in which they do it.

The main constructs put to use by Hough are those of Barrington Moore's "rational-technical society" and the conventional "monistic" conception of Max Weber's model of organization. These are critically examined in the context of Soviet industrial decision-making to ascertain both to what degree they explain the political processes operative there and to what extent they are realistic models for the conduct of industrial administration and development administration in the U.S.S.R. and elsewhere.

The author states that Soviet society must . . . continue to deviate from the "rational-technical" model in at least three significant ways: the existence of illegal administrative practices, the participation of local Party organs in the making of industrial decisions, and the execution of political power functions by local Party organs. In addition, Hough criticizes the model for not taking into account deviations which must exist in any society and for neglecting certain important political phenomena. As for the Weberian view of organization, Hough argues, as have Simon and several other scholars, that deviations from its specifications are necessitated by the demands of an effectively functioning organization, and not only for the service of special interests, e.g., the power needs of the Communist Party. These arguments lead to another, more specific, thesis: area coordination by the local Party organs is a vital necessity for effective development administration (the Soviet state has long been engaged in a variant of development administration through the rapid and large-scale advancement and expansion of industry). The argument tying together these theses is that economic development and the expansion of industrial capacity and production is a nonroutinized process in which the values of precision, predictability, legality, and orderliness are outweighed by those of "creativity, initiative, and discretion." (p. 296)

This argument is derived from a realistic analysis of the actual ways in which the Party's local-level organs function vis-à-vis industry. Hough convincingly argues that the existing deep Party involvement in industrial matters is necessary for industry to function as intended, particularly in matters such as timely delivery of supplies and other aspects of cooperation among plants and organizations, e.g., the enforcement of contracts. The author contends that these operations on the part of the local Party organs are not capricious or excessive, but are limited, in effect, by conventions which became established as early as the 1920's and also by various objective factors, e.g., lack of time in relation to the Party organs' many other responsibilities, and the relative imbalance of knowledge and experience between the Party "coordinator" and the managers he supervises. Thus, the Party's "marginal and adjusting" (pp. 254–5) work in industry is a regularized and "normal component" of its "executive responsibilities" (p. 171) which is objectively "useful" for the production process and also for the protection of regional interests (p. 270).

Hough's position is correct within the framework and perspectives he has adopted. However, if these were replaced by others which are also pertinent, the conclusions would not be so favorable to the Party's role. I agree that the "invisible hand" is not allowed to operate anywhere at present (p. 196) but does even the most effective functioning of industry justify the regulation of industrial life by a single organization which monopolizes the political power of the system. The local Party organs often function to realize the main goals of industry, but at a great, almost disabling effort and at a high cost in the initiative of the managers and the functioning of the formal organizations of industry, problems the present economic reform has not eliminated. Also, the acquisition of supplies in both the U.S. and the U.S.S.R. is indeed at times worked out informally, but there are political differences between the systems which outweigh these and other similarities in the operation of the economies. In other words, although the C.P.S.U. often aids in the effective functioning of industry, it is not exercising a benign influence throughout society or in all aspects of industry. Some of the major difficulties of the Soviet economy and society are systemic in nature and cannot be eradicated by this institution with its political stake in the system as it is. The author's arguments thus constitute an explanation and justification of only one aspect of the Party's role.

However, the book performs a real service in its effective destruction of many of the prevalent simplistic views of the Communist Party and its role in the economy. Hough shows that the meaning of the word "Party" varies with territorial level, function, and even situation. Also, he clarifies the meaning of the term "Party apparatus" used so indiscriminately and shows the apparatus to be a variegated group in terms of education and experience. In addition, he clarifies the role of the local Party organs in industry and their relationship with state agencies. The appendices dealing with the membership of Party committees and bureaus at the republic and *oblast'* levels and the listing of 170 major plants are an important bonus. The value of the book is enhanced by the results of interviews conducted in the U.S.S.R. and data gleaned there from some local newspapers which cannot be exported.

*The Soviet Prefects* is an up-to-date, well researched, and thought-provoking addition to the few excellent books on the Soviet political system.

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*How Communist China Negotiates.* BY ARTHUR LALL. (New York: Columbia University Press, 1968. Pp. 284. $7.95.)

*Negotiating with the Chinese Communists: The United States Experience, 1953-1967.* BY KENNETH T. YOUNG. (New York: McGraw-Hill Book Company, 1968. Pp. 443. $10.95.)

A reader of this review will benefit from a brief score card. Kenneth Young is a former U.S. official and diplomat who negotiated with the Chinese at Panmunjon and is now president of the Asia Society. Arthur Lall is a former Indian diplomat who represented that country at the 1963 Geneva Conference on Laos and is now teaching at Columbia. The writer is a former U.S. Government official, now at the Brookings Institution.

None of us can, and indeed, in this particular case none tries, to escape our backgrounds. Lall's book is much more a memoir of the 1962 Laos conference than a general discussion of Chinese negotiating strategy. Young's study reflects not only his own experience, but also that of his colleagues in the Department of State, and largely reports on the bilateral ambassadorial talks between the United States and the Peoples' Republic of China. Thus, despite the general titles, the two books focus largely on specific aspects of the Chinese diplomatic experience, although they raise issues even beyond that of the question of Chinese negotiating style and strategy.

Much of each of the two books is devoted to a careful reporting of public and private events. A reader who wished to know what was said by representatives of the United States and China and the other participants in the Laos conference, or what the two countries said at the ambassadorial talks will find no better sources. Each book is based on inside information. Lall, as the Indian representative at the Geneva Conference, is able to report much detail not available from other sources. Young's study is equally rich. He claims that the book is based on press leaks and other published material. However, there is apparently more to it than that. At one point, for example, he notes that the press stories were inaccurate, and there is no doubt that this study reflects a knowledge of the U.S. government records of the conference.

Unfortunately, the books offer little that is useful beyond the descriptions of events, although both authors do present brief discussions of general Chinese attitudes towards negotiations. What they offer is not particularly helpful in explaining Chinese behavior even at the meeting they discuss, or in predicting how the Chinese are likely to behave at future conferences. Lall, for example, tells us that the Chinese demonstrate flexibility based on their belief in inherent contradictions and are prepared to compromise in the short run because of their focus on long-run objectives. Young also reports that the Chinese have a long historical perspective. He notes that they view negotiation as an adversary process and that they place little value on the dignity of man. Such generalizations do not carry us very far.

Two larger questions are raised and at least implicitly dealt with in the two books. The first concerns the role of diplomatic negotiations in affecting the behavior of nations beyond the conference table. The second concerns the evolution of U.S.-Chinese relations since the establishment of the Chinese Peoples' Republic in 1949.

Anyone who spends any large part of his life engaged in a particular activity comes to believe in its importance in influencing the course of events; and authors need to feel that their subject is critical and self contained. Lall and Young are no exceptions. Having sat through the 1962 Laos conference, Lall clearly believes that it affected events in important ways, and Young is equally convinced of the valuable role played by the bilateral ambassadorial talks in shaping U.S.-Chinese relations. Lall also assumes that negotiations should lead to agreements. He leaves the reader in little doubt that their job is to carry negotiations to a successful conclusion. But, how important was this "successful" negotiations in affecting behavior?

The critical decisions regarding Laos had been made by the American government prior to the opening of the conference. President Kennedy had apparently concluded that he would not commit U.S. groundtroops to Laos to prevent its being overrun by North Vietnamese troops. He then charged Harriman with, in effect, negotiating the best arrangement that could be worked out, which hopefully would have the effect of discouraging a North Vietnamese attack. On the other side of the table, the Russians apparently were prepared to exercise some pressure on the North Vietnamese because of their concern that the Laotian situation would adversely affect the general trend of Soviet-American relations. China was not anxious for a military clash near its borders at this time. The conference painstakingly worked out an international agreement on the neutrality of Laos to go along with the political sentiment among the Laotians themselves, which was negotiated separatedly. The details of the agreement may well have been influenced by the skill of the negotiators, and it of course is possible that the negotiations would have failed if not for the determination and energy of Lall and others in constantly bringing the parties together. Nevertheless, the basic story concerns the internal politics of Laos and the calculations in the United States, the Soviet Union, and China, both of which fall beyond the scope of his narrative, as Lall has defined it. Lall also does not concern himself with the implementation of the decisions of the conference and the future evolution of Laos. It is clear beyond a doubt that the North Vietnamese almost completely ignored the agreement to withdraw all foreign trooops from Laos, and that within a few years the United States was also in violation of the agreement. A tripartite government ratified at the conference came into existence only on paper, and Laos very quickly fell into a situation of de facto partition.

How the situation on the ground in Laos would have differed, if at all, if the conference had not been held or it is ended unsuccessfully, is an interesting and important question with which Lall does not deal. Nor does he consider how Chinese behavior on other questions or the attitude of the United States towards China was affected by the results of the conference. If international negotiations of this kind are important, they are so only if they affect what actually occurs in the territory negotiated about, or the perceptions of the leaders of major countries about the likely behavior of other countries in the future. The Laos conference of 1962 did not have any major consequences. Nor, in fact, does Lall argue that it did.

Young explicitly tries to make a case for the central importance of these bilateral talks in shaping U.S.-Chinese relations. In particular, he seems them playing a critical role in the offshore islands crisis of 1968. Here again, however, the self-imposed limitations reduce the value of the work. Mr. Young does deal briefly with the general crisis situation, relying primarily on secondary sources, and then reports in detail what was discussed in Warsaw. However, since he does not consider the other moves being made by the parties, he falls into the trap of greatly overestimating the role of the talks. He also misses what I believe to be the key point; namely, that the Chinese asked for the talks as a way of reducing the likelihood that the U.S would overreact to its probe by attacking the mainland, and that the U.S. agreed to talk, because given the intensive domestic opposition to the U.S. involvement, it had no choice.

On the broader question of U.S.-Chinese relations, Mr. Lall has little to say, and Young's treatment is handicapped by an attitude common to semi-official discussions of such issues—the assumption of moral rectitude. Implicitly, and at times explicitly, he proceeds as if the United States was seeking to improve relations, was acting from noble purposes, and was negotiating "sincerely." At the same time, Young points out Chinese acts of duplicity, the unreasonableness of Chinese behavior, and their constant efforts to use the talks for their own ends. At one point, in discussing the period 1954–55, when the Chinese were seeking limited steps to improve relations, and the United States was resisting them, Young does allow that the United States may not have acted as forthrightly as it might have in seeking improved relations. In writing from this perspective, Young gives an accurate picture of the attitude with which bureaucracies approach their relations with other countries. Most outside observers would argue that a major chance to improve relations was deliberately passed over.

Bureaucrats assume that the objectives of their nation are consistent with a higher good, and that the other side's sincerity is open to question. More important, they resist analyzing the other side's behavior, taking into account that its bureaucrats believe just the opposite.

But Young's framework is distorting. The U.S. and China alternated in seeing the talks as useful and in favoring limited steps to increase contact. For both governments these were decisions taken in the context of their overall objectives and were basically tactical. During most of the period covered in Young's book, the United States was at least hoping for the overthrow of the PRC and was negotiating mutual security treaties with China's neighbors; it was without supporting a rival government on Taiwan and building up its military strength in the Far East because of a widely shared belief in the U.S. Government that the Chinese leaders were aggressive and expansionist. The Chinese leaders, fearful of an American attack, were supporting the North Vietnamese and, in general, seeking to reduce U.S. influence in Asia.

Given the interests and suspicions of the leaders in Washington and Peking, there was in fact little that could be negotiated, and it is difficult to believe that small steps could have made a big difference unless they led both governments to substantial changes in their images or policies. We still do not know enough about the internal debates within China about foreign policy to know whether a different U.S. posture at Warsaw would have affected Chinese behavior regarding control of nuclear weapons, support for the Vietnamese insurgency, or relations with her neighbors, but one may doubt that any of these would have changed. On the American side, one needs to refer only to Eisenhower's warning to Kennedy that he would denounce him if he moved toward recognition of "red China," to doubt that a different scenario at Warsaw would have had any real significance. Whether negotiations of a different kind, or on different subjects, or the small steps which might have come from them could have changed the misperceptions which lay behind at least some of the presumed conflict is difficult to predict.

In short, the two books provide useful data, either for a diplomatic historian's reporting on Chinese relations with the outside world, or for speculation about how negotiations affect the behavior of nations; but in and of themselves, they provide little real insight into the effect of negotiations on the events that they are about, or into U.S.-Chinese relations.

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# BOOK NOTES

## POLITICAL THEORY, HISTORY OF POLITICAL THOUGHT, AND METHODOLOGY

*Tocqueville and the Problem of Democracy.* By Marvin Zetterbaum. (Stanford: Stanford University Press, 1967. Pp. 185. $5.75.)

Few books on the subject of American or modern democracy can rival the youthful Tocqueville's *Democracy in America* in profundity and comprehensiveness. So preoccupied with lesser matters are political science and education today, however, that the bulk of the work is rarely read. Zetterbaum's book, along with recent studies by Richter, Drescher, Lively and others, should help make such ignorance less pervasive. Showing first that Tocqueville's central theoretical problem is the problem of democracy, it then reconstructs his reasonings, relates them to the problems and themes of classical and modern political philosophy, and finally evaluates them.

Zetterbaum's initial task is to penetrate beneath Tocqueville's apparent refusal either to announce in favor of democracy or to weigh democracy and aristocracy against each other. The democratic tide is called inevitable, long-prepared, divinely-inspired. But Zetterbaum marshals impressive evidence, especially from Tocqueville's private writings, to show that he does not seriously believe in historical inevitability. He suggests that Tocqueville uses this idea to conceal his own partisanship while moderating both the democratic will to destroy and the aristocratic will be resist as they raged in bitter struggle around him. In fact, the core of Tocqueville's political philosophy lies beneath his superficial impartiality, and consists in his assessment of these two opposing regimes. Ultimately, as Zetterbaum argues, it is because he favors democracy over aristocracy in crucial respects that he can encourage his contemporaries to accept its tide as inevitable. And it is his appreciation of aristocracy that permits him to understand the nature and limits of democracy and to encourage those forces more apt to preserve and elevate it.

Zetterbaum is able to demonstrate that Tocqueville follows certain moderns in his view of the natural equality of men and in considering the active consent of the governed, or democracy, essential to the protection of the natural rights of all. As for aristocracy, Tocqueville seems in the end, but for reasons not made perfectly clear, to identify it with actual caste or inequality based on birth rather than with the theoretical rule of the best as such, and therefore rejects its oligarchic character: only democracy is capable of pursuing the common good or the good of most. Yet the heights of political and human greatness are to be found in aristocracies, not democracies, and Tocqueville reveals a fuller or more open appreciation of this greatness perhaps than any modern before him. Nevertheless, Zetterbaum tells us, he did not hope to engraft the virtues of aristocracy onto the fundamental justice and humanity of democracy. Instead, the problem as he saw it was to draw from the democratic ethos itself those qualities and correctives that might increase democracy's stature and alleviate its ills.

What Tocqueville had in mind was modern democracy, which appears, at first, to be unequivocally superior to its ancient counterpart. Without slavery it has become more just, and with larger territory and population (using representative government) it has become more stable. On the side of the ancients, however, remains their political virtue or patriotism—an "instinctive" love of country and readiness to sacrifice for it. This admirable dedication Tocqueville tries to simulate through the modern "principle of self-interest rightly understood"—*i.e.*, through enlightened selfishness.

The crux of modern democracy Tocqueville considers to be equality even more than liberty—both of them principles already discerned in ancient democracy by Aristotle. He also apparently places a somewhat novel stress on "social condition" as the determinant of a country's politics. Equality is the social condition of modern democracy: the relation of man to man in fact as well as norm precludes fixed hierarchical arrangements, and an egalitarian political life is but one element within a more general eqalitarian fabric.

In Tocqueville's portrayal, a democratic society of independent equals engenders the separateness and isolation of individuals and families, encourages each in a competititve quest for material well-being, and—while spreading compassion—drowns heroic conceptions and conduct in mediocrity. The extreme individualism of modern democracy, urging every man to examine and judge his own beliefs, Tocqueville seems to trace to the modern philosophical revolution begun by Bacon and Descartes. He fails to note, however, that the peculiar commercial or capitalistic spirit characteristic of modern democracy also has its root in modern natural and political philosophy rather than in democracy per se.

The problem of democracy derives from its essential egalitarianism and individualism. Because of them, democracy threatens to destroy greatness and social cohesion, and since the power of egalitarianism is ultimately greater than the power of individualism and liberty, democracy can turn into despotism, and may even do so without appearing to. Not only does the influence of public opinion dominate both masses and educated alike, but the tendency of democracy is increasingly to make the state, viewed as a popular instrument, the main provider of equality, and well-being for all the people, especially for the poor majority. If we may interpolate, the coming of what today is called the "welfare state" will mean the extinction of independence in the electorate and hence of real freedom.

To offset these existing and prospective ills, Tocqueville wanted to find a place within democracy ". . . for liberty, for human excellence, for a renaissance of public virtue, and for the possibility of greatness." Zetterbaum's analysis of the way these emerge from the democratic ethos is fascinating. Thus, equality gives rise to the desire to rule oneself and hence to the necessity of cooperating with others in democratic self-rule, whether through local government or a variety of political and non-political associations. The self-care of each, properly understood, makes each man realize he best cares for himself by caring for the public good; it issues in a kind of rational or calculated patriotism. And religion, also interpreted as involving each man's concern for his own (eternal) well-being, helps restrain vulgar selfishness and provide a sense of futurity and of higher devotions. Even commerce extends the self-care of the individual in such a way as to bring forth not only the solid virtues of bougeois society but a kind of independence and energy as well.

In his last chapter Zetterbaum wonders whether Tocqueville's solutions are successful and whether even he fully believed in them. Can "self-interest rightly understood" serve as the underpinning, however far removed, of freedom, patriotism, morality and greatness? Can a nation long endure on its basis? Tocqueville had his doubts, as Zetterbaum indicates, and was partly of the opinion that freedom, the nation and virtue need, and deserve, to be loved for themselves. The reader, then, must either assume, along with most of our profession, that the proper political and extra-political institutions obviate the necessity for any high moral and intellectual qualities among the leaders and people of a democracy; or he must conclude that democracy requires an admixture of virtue in the old and strict sense, and cannot succeed without having at its foundation and in periodic renewal certain aristocratic elements (especially education) to overcome the inherent defects of the modern principle of self-intercest.

In this book Zetterbaum has brought us face to face with Tocqueville's central problem only after penetrating beneath certain forms of concealment. He has not permitted himself to be distracted by lesser issues, biographical or historical; he is properly reluctant to charge Tocqueville with the sort of error only lesser minds are likely to commit; and he never presumes that he possesses, in present social science, a superior vantage point. The result is a major advance in Tocqueville scholarship. Nevertheless, one has the impression by the end of the book that certain relevant issues are left unsettled or unexplored. In part because no strict definition of "social condition" is essayed, Zetterbaum's attempts to compare Tocqueville's understanding of the relation between political and social matters with that of the ancients and other moderns remain unclear. And if Tocqueville really believes in the primacy of politics, as Zetterbaum contends, does he not damage the prospects for political greatness by the weight of his stress on social condition? It would also have been helpful if Zetterbaum had more systematically distinguished between what Tocqueville says about democracy (meaning modern democracy) and what can be said, or had earlier been said, about democracy per se. Finally, some additional attention to Tocqueville's conception of virtue, and to his overall relation to ancient and modern political philosophy is necessary before the reader can judge the claim that ". . . despite its novel elements, Tocqueville's program probably would not have struck even a classical political philosopher as unique. . . ."

Despite this incompleteness, Zetterbaum has gone a long way toward vindicating Tocqueville's reputation as a modern master, and thereby, we may add, toward raising the current level of discussion in American and comparative politics to within hailing distance of what it was more than a century ago. For if democracy needed a Tocqueville to explore its problems then, it is certainly in greater need of one now, and a political science incapable of sensing this has lost precisely the perspective that Tocqueville, among the moderns, is most capable of supplying.—DAVID LOWENTHAL, *Boston College*.

*The Democratic Experience: Past and Prospects.* BY REINHOLD NIEBUHR AND PAUL E. SIGMUND. (New York: Frederick A. Praeger, 1969. Pp. 191. $5.95.)

In an early essay "On the Interpretation of *Weltanschauung*" Karl Mannheim distinguishes among three levels or dimensions of meaning in terms of which a cultural artifact can be interpreted: the "objective," the "expressive," and the "documentary." Interpretations of the first sort require us to refer the artifact to the standards and criteria of the *genre* to which it manifestly belongs; "expressive" interpretation involves an account of motives and intentions; and "documentary" interpretation sees the artifact in relation to more general patterns of social and cultural developments, it proceeds, in short, in the way that historians normally proceed with documents—as evidence about the way the world was at a time and in a place. There is a strong presumption that a review in a scholarly journal will operate at the first level of meaning: appropriate canons of theoretical adequacy will govern the interpretation and assessment of a book. That presumption cannot be overridden, I would imagine, in favor of the second level. If there is nothing worth discussing about a book except the intentions of the author and the personal needs satisfied in writing it, the book does not merit review. But some books are most interesting as documents and it will happen that discussion of some books can best contribute to critical self-awareness within our discipline if it pursues that interest.

The book under consideration here falls into this latter class. There are three parts of unequal length. In the first part, Reinhold Niebuhr interprets "The Democratic Experience in Western History"; in the second part, Paul E. Sigmund canvasses "The Prospects for Democracy in the Developing Areas" (within the span of eighty pages, there are chapters on Africa, the Middle East, Latin America, and Asia); and the last brief section contains a sketchy bibliographical essay in which Sigmund cites parallels to the basic contentions in the text in the writings of Robert Dahl, Edward Shils, Seymour Martin Lipset, Harry Eckstein, and a few others. The basic contentions of the book can be quickly summarized. Speaking of the anti-Utopian proponent of democratic government, Niebuhr says:

> His approach to the political order is characterized by a mature empiricism and realism. He knows that there are many forms of power, not merely one. He knows that there must be an equilibrium of social power as a basis of justice and he has developed a highly complex political organism with many forms of countervailing power. He also knows that the political authority which must establish the order of the community must be one, but that he must triumph over many forms of subnational communities to achieve this wholeness of authority and be able to speak for the whole community. (13)

The democratic experience, according to Niebuhr, involves three basic accomplishments: the creation of national units, the social and ideological constitution of "responsible individuals," and the attainment of some sort of social equilibrium. Developments in states not viewed as participants in that experience are assessed in terms of prospects for success in these regards. The perspective offered has a certain plausibility and the essays as written occasionally contain provocative suggestions, but it remains a thankless job to attempt a theoretical assessment of the argument (or to proceed at the "objective" level).

It is hard enough in any case to find agreement upon objective standards to which an interpretative essay can be held. But some guides are available. There must be a certain integrity of language. Niebuhr inveighs constantly against "Utopian" ideologies and perspectives, but constantly proclaims the "triumphs" which constitute the Western experience. Some would consider a history punctuated by triumphs as pretty Utopian indeed. The crucial term "equilibrium" is used so flexibly that there is no kind of relationship between forces which is not somewhere so designated. And so on.

A second demand often accepted as a general guide to interpretive writing is that it "orient" us to our situation, that it enable us to make the kinds of distinctions which we feel called upon to make in the course of our practical life, that it call attention to facts and forces which impinge upon us. This is a standard to which the authors appeal, I believe; but it is not a standard which can be applied without great disappointment. We get no help with the conflicts which mark contemporary prosperous nations; we get no help with their wars (it would be instructive, for example, to hear Niebuhr try to put his vigorous opposition to the war in Vietnam into the language of his essay); and we get very curious accounts of past and recent history in Germany, Katanga, and elsewhere. An attempt to encounter the book at an "objective" level involves a thankless falling through open doors. The argument simply cannot be taken seriously in this way.

The book must be seen as a document of the time when American social science enlisted without much opposition or discomfort as an instrument of cold war policy. The genesis of the document is instructive: Sigmund had but recently completed a tour of duty with an intelligence agency when he and Niebuhr gave a joint course at Harvard almost ten years ago; Kenneth Thompson of the Rockefeller Foundation fostered a publication built upon that course; and Praeger published it. The circumstances and cast of characters are almost ideal-typical. There are nice questions about publication of this old manuscript at this time; but they are matters for speculation. The point is not any moralistic impugning of motives, but identification of a historical complex which helps us to understand the document and which is in turn further illuminated by it. Niebuhr writes:

> Forswearing a utopian interpretation of our cause has the advantage of preventing disillusion and hysteria if, in the worldwide struggle, some of the nations allied with us do not reveal the resources necessary for the achievement of a healthy democracy. We will not be too disturbed by evidences of inadequate solutions to the problems of the responsible exercise of power. We will know that dictatorships are dangerous and that all irresponsible power creates abuses. But we will also know that in a world of change, governmental experiments are reversible if they are not controlled by a tight dogmatic system. (14)

It is not necessary to question the sincerity with which Niebuhr goes on to hope for a time when Communist dogmatism will give way to pragmatism and of his opposition to a crusading war; but we must note that the perspective sees only one conflict as ultimate reality and sets out to attack only that "hysteria" which could undermine the morale and unity of "our" side. The commitment expressed in such an enlistment may be defensible on rational moral grounds, as an act of responsibility. But the politics of that political and social inquiry are not longer persuasive to many and they certainly are not inherent in objective and empirical social research as such. The book is a document useful to the development of our discipline because it can be usefully studied as we proceed to clarify the relationships between ideology and theory.—David Kettler, *The Ohio State University.*

*The Political Theory of John Dewey.* By A. H. Somjee. (New York: Teachers College Press, 1968. Pp. 208, $7.50.)

John Dewey addressed himself to many of the same problems which still preoccupy us: the role of science in studying and evaluating public policy and politics, the dimensions and purposes of planning, the nature and validity of democracy, the manner in which we identify and assess the hitherto unexpected but consequential effects of policy, and the relationship between political activity and psychological growth. Dewey can be considered a systems-builder and analyst who moved away from the black box and facile feedback loops that purport to explain causality. Insistent that we focus on the consequences of public policy, Dewey advanced into the uncertain world of multicausality, deferred effects, and uneven impact. The problems and challenges of Dewey's theory lie at this complex level.

While Dewey failed to provide a managable set of standards for the evaluation of public policy, he promoted a temperment and method which is still relevant to political scientists. For Dewey, political analysis was more than just the advancement and study of values or the scientific examination of behavior and policy: they were hopefully to be synthesized in an effort to make the study of political phenomena both empirically based and socially relevant.

Professor Somjee in *The Political Theory of John Dewey* has undertaken the important task of examining Dewey's concepts of experience and common inquiry and relating them to democracy and planning. The author also seeks to examine the roots of Dewey's political theory and spends considerable time with several European theorists of the late eighteenth and nineteenth centuries. But Somjee, while providing several helpful insights, does not capture the dynamic quality of Dewey's thought. Somjee, who is expanding his doctoral dissertation written under Michael Oakeshott, examines the logical consistency of Dewey's theories and their relationship to both other theorists and the philosophy of science.

The section comparing Dewey with other writers contains several new interpretations, particularly the discussion on Dewey and Kant, but the author fails to draw a new perspective after he offers a corrective to earlier studies. Somjee often introduces stimulating themes without developing them. For example, he correctly recognizes that although Dewey owes a heavy debt to James, he was not simply an extension of James. However, Somjee based his conclusions on a few critical letters by Dewey and "his studious avoidance of James's principal ideas." (p. 57) It would have been informative to have explored the differences between these two American writers in a more systematic manner.

Dewey envisioned a scientific approach to comprehensive political planning. He distinguished between the application of the scientific method in the social sciences, which would lead to the expansion of knowledge, and the wholesale borrowing of scientific concepts appropriate to one field as necessarily applicable to all others, which would only lead to a "form of absolutist logic" as the social Darwinists had earlier demonstrated. Although Somjee accepts Dewey's distinctions, he nevertheless insists that the scientific method is applicable to the study of public policy only when incremental reforms are undertaken. Comprehensive planning cannot be sufficiently "cautions, well-thought-out, and manageable," and "the harm done by any errors may . . . be irremedial." (p. 155) There are many reasons for preferring marginal change to major reorganization, but the alleged scientific integrity of the former over the latter is not substantiated here. In some situations, if only "trial-and-error" reforms are introduced, dysfunctional factors may intensify beyond a threshold where even more sweeping proposals are demanded. In other words, Somjee's "trial-and-error" reforms may lead to "irremedial" conditions.

Because of his preference for stability, Somjee offers some arresting notions about science and government. We are told that science requires a stable political system and that Dewey's program would produce an untenable milieu for the scientist. (p. 159) Part of Somjee's problem is that he does not distinguish between possible levels and areas of planning, simply juxtaposing his penchant for stability against Dewey's more comprehensive programs for change. But there are different magnitudes of change, and Somjee would have performed a substantial service had he focused on the relationship between instability and growth instead of offering postulates about political order and scientific advancement. Indeed, many of Somjee's problems stem from his decision to ignore one of the essential elements in Dewey's thought-growth. Dewey saw that what might contribute to growth at one period may endanger it at a later time. Furthermore, change entailed possible disadvantages as well as opportunities for certain types of growth, but so did the maintenance of existing arrangements. Choices between the present arrangment and other options could only be made after an exhaustive study, according to Dewey.

The task for political theorists, according to Dewey, is neither to find the totally painless way to a better society, which being an impossible enterprise means we never move, nor simply to identify the ideal polity and rush to achieve it, which means we move without any consideration of the risks or costs involved. Rather, the task is to specify the democratic values we seek to promote and the effects of programs on individuals in terms of these values. However, these problems are beyond the scope of *The Political Theory of John Dewey* and await exposition and analysis elsewhere. Professor Somjee's book can be recommended both to those who wish to find suggestive comparisions between Dewey and several of his European contempories and to those who are unfamiliar with Dewey's epistomology and its relationship to Dewey's concepts of democracy.—RONALD J. TERCHEK, *University of Maryland.*

*Progress and Disillusion; The Dialectics of Modern Society.* BY RAYMOND ARON. (New York: Frederick A. Praeger, 1968. Pp. 230. $5.50.)

In this book, Raymond Aron identifies and examines the major forces which he sees at work in the modern world. His analytical method is dialectical, in the sense that he confronts various ideals with various aspects of reality, a method which he had employed before, particularly in *The Opium of the Intellectuals,* in which he assessed marxist ideology in the light of the conditions of industrial society, and in *Espoir et Peur du Siècle,* in which he performed the same operation for the elements of conservative ideology.

The ideals which Aron discusses, and which he regards as inseparable from modern civilization, are equality, personality (or individuality) and universality. The first two ideals are familiar; universality is less so. He seems to regard unviersality as the organization of humanity into a single society, "the unification of mankind on a planetary scale" (p. 165), although it is not entirely clear whether he is thinking of a common political system or of the kind of economic equality among nations which would make possible a common mode of existence.

Some of the realities against which he sets these ideals spring from what Aron calls "the Promethean ambition—the urge, to borrow a Cartesian expression, to become the *masters and possessors* of nature" (p. 216), and to which he thinks the ideals may be subordinate. This ambition has produced the science, technology, industry, wealth and bombs which condition the national and international social orders in which men struggle to attain their ideals. But hardware are not the only social facts, and the realities with which Aron confronts the ideals also include social elements which are not directly related to the Promethean urge, such as family, school, culture, race and nation.

The book is divided into three parts. Part One, entitled "The Dialectic of Equality," explores aspirations toward equality within the context of the various forms of inequalities which characterize modern societies as the result of class stratification, oligarchic aspects of the distribution of political power, racism and ethnic conflicts.

Part Two, entitled "The Contradictions of Socialization," discusses the drive for personal fulfilment in the light of the constraints imposed by socializing agents: family, school and mass media, as well as the constraints of hierarchy and discipline imposed by economic systems which are impelled either by a gospel of production or the goad of competition. The theme of the second part mixes with that of the first, as Aron discusses the impact of industrialization on the family itself.

Part Three, called "The Dialectic of Universality," contrasts the unifying forces of transportation and communication, the growth of English as the lingua franca of the planet, and the common application of science and technology with local indifference to distant concerns, the jealous assertion of national sovereignties, unequal rates of economic development, and indeological conflicts both among marxist-inspired nations and between them and the democracies.

The book has a broad sweep. In an attempt to define the distinguishing features of modern society, Aron has produced a sort of inventory of the forces making for conflict and unity both within and between nations. One great merit of the work is its effort to see modern society whole, to avoid partiality of analysis. Aron's sensitivity to all the currents at work in modern society is sufficiently great to have produced this statement, written before the French revolt of May-June 1968: "The indifference which so many citizens show toward their state and their rulers in countries where there is representative government. . . . probably attests to a latent uneasiness which might suddenly explode in a crisis of wild passions" (p. 128).

But the price of comprehensiveness (within a comparatively short book) is that some matters must be dealt with too briefly. I feel that this is true, in particular, of Aron's treatment of the impact of mass media on social and political behavior. Is it correct to say that even though it is now possible to convey information rapidly about distant places and events, "none of this . . . affects the average viewer or reader as much as a quarrel with a neighbor or a colleague at the office, or the fluctuations of his own personal fortune" (p. 138)? This seems to me to understate the significance of the mass media with respect to such diverse matters as the nationalization of French politics and the evolution of American attitudes toward the Vietnam war.

But for each point which one may doubt, there are many more which impress. Aron has more ideas to the chapter than most people place in a book, and there is a concreteness and a problem orientation to the book that keeps it close to our current preoccupations at the same time as it places them within the three central analytical categories which provide the framework for the book.—ROY PIERCE, *University of Michigan.*

*Soviet Perspectives on African Socialism.* BY ARTHUR JAY KLINGHOFFER. (Rutherford, N.J.: Fairleigh Dickinson University Press, 1969. Pp. 276. $8.00.)

Most studies on Soviet views toward Africa have revealed more knowledge about the Soviet Union than the new African states. This book is no exception. In it, the author summarizes Soviet ideological writings about African socialism between 1955 and 1964. He focuses on the ideas mainly of the late dean of Soviet Africanists, Ivan Potekhin, and on the West African states of Ghana, Guinea, and Mali. The work makes several contributions. Professor Klinghoffer has a comprehensive knowledge of Russian language sources. He points out the significance of the Cuban revolution. At least for some Soviet ideologues, the Castro take-over in 1959 seemed to foreshadow other territories in the Third World becoming Marxist-Leninist states. In his concluding chapter, the author emphasizes the ambivalent, fluid, and pragmatic perspective taken toward African socialism. Soviet support for some ideas of African socialism stemmed from African opposition to capitalism and Western colonialism. Yet, the Russian realized that most Africans refused to identify socialism with "scientific socialism," Marxism-Leninism, or the Soviet interpretations. In forgein policy matters, African leaders did not always support the USSR's position. Domestically, no African states were ruled by a highly organized, centrally disciplined Leninist party. Like many Americans, the Russians often seemed confused about the fluid African situation. They lavished most attention on Ghana, Guinea, and Mali, presumably the states most sympathetic to the Soviet position. Yet their views toward Nkrumah, Tourè, and Keita changed between 1955 and 1964. Professor Klinghoffer concludes that the Soviet position toward specific African states depended mainly on African foreign policy stands, not so much on internal social structure or domestic economic policies. If African socialists supported the Soviet foreign policies, then the Russian perspective tended to be more favorable. However, the Soviet writers definitely did not show a monolithic stand. Divergencies of opinion existed between those writers expressing sympathy for the incumbent African leaders and those revealing greater hostility.

The main weakness of this study lies in its wholly descriptive nature. There is a need for more explanation, analysis, and interpretation. The book is too much a repetitious description of different views toward African socialism. Professor Klinghoffer also exaggerates the effect of Soviet ideology on African economic policies and political party structure. He over-estimates the control exercised by the single party in Ghana, Guinea, and Mali. Although citing the writings of Soviet specialist Zbigniew Brzezinski, he never mentions the revisionist work of Aristide Zolberg on African political parties. If the author had consulted Zolberg, Henry Bienen, or Clement Moore, he would never had implied (p. 38) that "the African socialist regimes may be sowing the seeds of totalitarianism." Lacking a comprehensive knowledge of the internal socio-political structures of Ghana, Guinea, and Mali, the author has confused ideological reality with the existential scope of power. When the "socialist regimes" can be easily overthrown by a military coup, as happened in Ghana in 1966 and Mali in 1968, then we have to assume that the single party lacks total control either to maintain the leaders in power or to initiate policies which would change social conditions. The main Soviet influence seems verbal, not organizational. Soviet and African interpretations of socialism use similar phrases. In actuality, the dominant Marxist influence came through the French Communist party and the Confédération Générale du Travail, not directly through the Soviet Union. Not Soviet economists but rather French economists with Marxist views have influenced some of the economic policies formulated by Guinean and Malian leaders. Greater attention to this source of influence, mainly found in former French West Africa, would have clarified the Soviet effects on Africa.

Professor Klinghoffer also does not take a too analytic view of African socialism. He tends to make misleading dichotomies about the ideas expressed by African socialists. For example, he distinguishes "parliamentary vs. revolutionary" socialists, moderate vs. radical socialists, nationalism vs. communism, and class vs. ethnic appeals. Yet these distinctions confuse more than clarify. The distinction between parliamentary and revolutionary socialists had some relevance to European states between the two world wars. Now even in Europe the revolutionaries have become part of the parliamentary system. Especially in West and East Africa there have been few real "revolutionaries" in the Soviet sense, largely because the French and British reluctantly agreed to yield political power to the African nationalist movements. Except in Algeria and in southern Africa, independence came largely through non-violent means. Moreover, the African legislatures have never possessed the power exercised by European parliaments. African power resides in the top executive leaders, the civil service, and the military. The author also does not seem to sense the relationship between class and ethnic group. He labels as ridiculous the Soviet interpretation that particular economic classes dominated the Nigerian and Kenyan states. Yet non-Soviet American scholars have made exactly this point. Relative to other states in West and East Africa, Nigeria and Kenya are rather economically well developed. Here class and ethnic ties reinforce each other; that is, particular ethnic groups have gained economic dominance and control of the government. The tensions between economic and primordial loyalties have partly accounted for the civil war in Nigeria. In short, the class and ethnic group explanations are complementary, not necessarily opposed. The same point can be made about nationalism and communism. A leader can be a nationalist as well as a communist, depending on the meaning attributed to communism. In one section (p. 175), Klinghoffer asserts that nationalism may be "a viable alternative to the development of communism in the African nations." In the conclusion, however, he implies that Guinea is headed toward some form of "national communism" (p. 247). Both assumptions depend on the definition of communism. Does he define communism economically (public ownership of the means of production), politically (total control by a Leninist party), or in reference to foreign policy matters, i.e., adherence to the Soviet foreign policy line or Soviet control of African nations through military and economic means? The meaning is not clear.

Besides the need to present a deeper analysis of the Soviet effects on African socialist policy, Professor Klinghoffer should have provided some complete explanations for the views taken by different Soviet specialists. He could have contributed greater background information about the scholarly institutes where Soviet Africanists carry out their research. Finally, the author offers no explicit explanations for the changes in Soviet perspectives toward African leaders. Presumably these changes reflect the current foreign policy line of the USSR, the changing distribution of power within the Soviet Union, the situation in the First World, and the fluid African conditions. The author mentions all these factors, but he needs to link them more explicitly to the Soviet ideological views. This information, as well as a more realistic assessment of the effect of Soviet ideas on African leaders, would have helped in transforming this descriptive account into an explanatory work.—CHARLES F. ANDRAIN, *San Diego State College.*

*Political Theory: A Conceptual Anlaysis.* BY JOSEPH S. MURPHY. (Homewood, Illinois: The Dorsey Press, 1968. Pp. xii, 267. $7.35.)

This book "is written from the viewpoint of analytical philosophy; and it construes political theory as an activity whose purpose is the clarification and explication of political concepts" (p. vii). The author seeks to analyze critically "the central and pervasive issues of political theory." This approach, it is claimed, differs from, and is preferable to, "conventional treatments," which are chronological or which focus on the institutional contexts in which political ideas arise. Dr. Murphy is "concerned with asking if a theory is true and adequate or false and deficient," and with stating reasons for his conclusions. The issues discussed are the meaning of the term "political theory," the ethical theory of natural law, the concept of the state, the concept of freedom, the relation between natural and social science, and the problem of values.

The author notes that "theory" in the study of politics has had a variety of meanings, including theory as metaphysics, theory as a science of behavior, theory as the methods and techniques of political manipulation, and theory as ideology. He suggests that due to an absence of agreed upon criteria of meaning, political theory has been more like philosophy or literature than like physics.

Plato, Aristotle, Hobbes, Locke, Margaret Mead, Jacques Maritain and Leo Strauss are considered as exponents of theories of natural law. Some trenchant criticisms of natural law theories are presented. For example, demands justified by natural law—e.g. for equality and for freedom—may not be fully compatible, but natural law does not provide us with criteria for resolving a dispute where such demands are in conflict. Murphy is also critical of attempts to support theories of natural law by reference to alleged cultural uniformities.

Throughout his book, Murphy offers criticisms based on logic and linguistic analysis. Occasionally his objections are trivial. For example, he cites St. Paul's dictum that every man in his heart knows "the law of God, which forbids man to sin, and commands him to do what is right." He then objects that "the logic of the words 'sin' and 'doing right' is such that it seems redundant to say that sin is forbidden." (p. 55). There may indeed be redundancy, but such an observation does not take us far toward an evaluation of natural law based on theology.

Cicero, Grotius and other exponents of natural law have described the *law of nature* as a dictate of *right reason.* Reason is thought to be man's defining characteristic and the foundation of his natural rights as a human being. But Murphy notes that reason in the narrow sense of the ability to abstract and generalize by the use of symbols varies greatly among men. Why, then, does the natural law theorist not conclude that the rights of men vary as their ability to symbolize varies? On the other hand, if reason is used in its broader sense to include expressions of intelligence not based on abstractions, it is no longer an exclusive attribute of human beings. Finally, Murphy points out that nature itself "does not and cannot provide us with standards or ideals" (p. 68). The author's discussion of the ethical theory of natural law is probably the strongest section of his book.

The author devotes two chapters each to an examination of conceptions of the state and conceptions of freedom. A contrast is drawn between holistic and individualistic theories of the state. The problem of determinism is considered, as well as positive and negative conceptions of freedom. Murphy also discusses the problem of explanation in the social sciences and concludes that "the scientific method appropriate to social inquiry is not significantly different on logical grounds from the methodology appropriate to the physical sciences" (p. 242).

In the final section of the book, the author returns to the problem of the ground of values. He asserts that "we must" find a "middle way" between "complete relativism and subjectivism" and an objectivism based on doctrines "metaphysical or theological in character." Dr. Murphy cites Stephen Toulmin in support of his "middle way," which involves emphasis on human reason. Murphy maintains that "what is best may be justified by an appeal to reason and found satisfactory if it is in accordance with the facts and if it is not at variance with the rules of logic" (p. 262). The position advanced remains sketchy, however. Problems, such as what happens when two conflicting alternatives each appear rational, and the possibility that individuals may have different conceptions of what is rational, are not dealt with.

Faced with the admittedly difficult task of evaluating major political and ethical concepts, the author too often merely summarizes what others have said and makes some minor points of logic, while leaving the important questions unresolved. This applies particularly to his chapters on the state and on freedom. In addition to expressing his own ideas more forcefully, Dr. Murphy might have relied less on lengthy quotations and improved the clarity and organization of the book.

This book unfortunately contains numerous errors in editing or printing including misspellings, word omissions and misplaced words. There is reason to believe such errors have been made on the following pages: 86 footnote, 87 footnote, 88, 98, 101, 105, 146, 147, 176, 177—three errors, 178, 261. The footnoting on p. 67 is inadequate.

The author has demonstrated considerable ability for critical analysis. He does not offer many original ideas, fresh answers or resolutions of the problems dicussed. The book, nevertheless, will be of value for graduate and undergraduate students who wish a review of the issues and ideas which concern philosophically-oriented political theorists. —Michael C. Stratford, *Central Michigan University.*

## AMERICAN GOVERNMENT AND POLITICS

*The Politics of Community Conflict: The Fluoridation Decision.* By Robert L. Crain, Elihu Katz and Donald B. Rosenthal. (Indianapolis: The Bobbs-Merrill Company, 1969. Pp. 269. $3.95.)

This book is billed as an advanced study in sociology, but Crain, Katz and Rosenthal present a political explanation of fluoridation decisions rather than a sociological one. The study uses statistical analysis of a large number (1,181) of fluoridation case studies, reconstructed from mail questionnaires distributed to city clerks, public health officers and newspapers editors in all American cities between 10,000 and 500,000 in population. Its authors have skillfully exploited their data to present a thorough examination of fluoridation decisions and to suggest some more general hypotheses about community decision making.

The point of departure in this study is the familiar alienation hypothesis: the assertion that local referenda, fluoridation referenda in particular, are defeated so frequently because citizens use referendum opportunities to express their alienation from the local political process. Crain, Katz, and Rosenthal take a larger view, arguing instead that the choice of decision making method in a community is a more important determinant of the decision outcome. When decisions are made administratively, they demonstrate, a "yes" decision is more likely than when the process of choice involves a referendum.

But referenda are uncommon decision devices even at the local level, so the authors next determine why some decisions are made by referendum and other are not. They show that the explanation is to be found primarily in the timidity of elected city officials. Fearful of incurring the wrath of any vocal minority, or unable to reach agreement among themselves, many local offícals fail to make an early administrative decision that would foreclose the opportunity for a referendum.

Fluoridation, the authors argue, is a complex and confusing issue to voters. Thus when a fluoridation decision does reach the public debate and referendum stage it is often defeated because voters react against an unclear scheme with uncertain risks and benefits, especially if elected officials do not take a strong stand in its favor. Finally the authors suggest that there is a city type—the "participative community"—where this sequence of events is most likely to occur. Such cities are identifiable primarily by their formal governmental structure, town meeting and nonpartisan mayor-counsil systems being most susceptible.

Multiple authorship may account for the one serious fault of this book, its confusing shifts in explanatory focus. For most of the book the alienation hypothesis is the target of criticism; suddenly in the last chapter it is supplanted by Kornhauser's mass society theory. But the research reported here is not really a direct test of Kornhauser's thesis. First it does not deal with national level systems, as Kornhauser does, but with limited function local governments. Second, it does not examine what happens under the conditions Kornhauser posits for a mass society; rather it challenges the validity of his assumptions about accessible elites and available masses. The author's point, that the timidity of political leaders has important consequences for the operation of a political system, is weakened by centering the discussion around Kornhauser.

The authors seem to have discovered part way through their research that they were investigating a phenomenon more general than fluoridation decisions. They begin with the decision outcome as their dependent variable, but this is soon replaced by the method of decision—administrative adoption, referendum, or "no action." Finally in the last chapter the former dependent variable, tendency toward referenda, becomes the independent variable and the explanation shifts from an analytic one to the construction of a plausibility argument for the concept of the "participative community."

Thus Crain, Katz and Rosenthal never directly confront the alienation hypothesis in the sense that they never attempt to predict passage or failure of fluoridation on the basis of political structure. Rather they show that passage or failure is strongly related to the kind of "track" that the decision gets onto, and then that the tracking of fluoridation decisions can be explained by looking at a city's political style and structure. It is still a matter for the reader's conjecture whether the tables running decision track against other variables are not hiding relevant variations because the "track" categories are not cleanly related to fluoridation success or failure. They do demonstrate, however, that mayoral support is important for success on both the administrative and referendum tracks.

One can also be skeptical about the feasibility of the research design because of the difficulties involved in getting the appropriate outcome data. Over half of the cities in the study fall into a category labelled "no action." In reality, this category encompasses several situations that it would be very useful to distinguish. It includes cases where a fluoridation proposal was never really articulated, cases of "non-decision" where proposals were never acted upon by the community's power structure, and cases of explicit administrative "no" decisions. These distinctions would be quite interesting to examine in light of the authors' theoretical explanations, but unfortunately the data were not available.

These criticisms aside, *The Politics of Community Conflict* is generally a persuasive and appealing book. Its authors' notion that city political leaders are expected to play a legitimizing role in important community decisions is a promising piece of theorizing which deserves to be explored further. As they are aware, of course, such a study needs other kinds of data, especially the role expectations that citizens have for their elected political leaders and for other community influentials.

The book's greatest value, however, lies in its perceptive treatment of the widespread phenomenon of the fluoridation controversy. If the assertion is correct that fluoridation decisions are merely extreme cases of a wide class of local political decisions, then the authors will have done social science a valuable service by looking beyond the issue-specific features of fluoridation politics to a more general understanding of community decision making. In any case they have presented a useful critique of the individual alienation paradigm, especially as it has been used to explain behavior, and they have reaffirmed the value of system level explanations for system level phenomena such as community decision outcomes.—ROBERT EYESTONE, *University of Minnesota.*

*Representatives and Roll Calls: A Computer Simulation of Voting in the Eighty-eighth Congress.* By CLEO H. CHERRYHOLMES AND MICHAEL J. SHAPIRO. (Indianapolis: The Bobbs-Merrill Company, Inc., 1969. Pp. 196. $7.50.)

A large number of propositions concerning the antecedents of legislative votes are brought together in this book and converted into a computer program for generating simulated congressional votes. At least 84 per cent of the resulting simulated votes agree with the actual votes cast, on 21 roll calls concerning the federal role, and 27 concerning foreign affairs, in the House of Representatives in 1963–64.

The simulation program consists of two major parts. In the *predisposition phase*, each representative is assigned a voting predisposition (toward yea or nay) on the basis of his party, state, region, constituency, committees, and positions taken by the President, signers of minority reports, etc. Those representatives whose predispositions fall outside certain limits are assigned yea or nay votes, while the rest then enter the *communication phase*. Each representative who enters this phase is assigned "conversation partners," including the President, legislative leaders, and the congressional rank and file. These assignments include a random element. As result of each simulated conversation, the predisposition of each representative who entered this phase is altered toward the predisposition of the partner. After all the "conversations," positive predispositions are converted to simulated yeas, and negative predispositions to nays.

To carry out the simulation, the investigator must code each roll call as to the types of support it is expected to receive, such as that from party, region, and constituency types. These judgments, while subject to error, are a necessary part of any such prediction; and as long as they are not based on the roll-call votes being predicted, they are not subject to fundamental criticism. But in the simulation, the investigator must apparently also estimate numerical values of several dozen parameters—contributions of numerous variables to the predisposition, or probabilities of conversation with various types of partners—and the procedure by which these estimates (or guesses) were made seems less clearly justifiable.

The assignment of values to these parameters seems a major weakness of this study, and indeed, of all simulation studies of its type, if they are alleged to be tests of hypotheses. Perhaps this difficulty can be made clearer if we compare the procedure with that of multiple regression. The predisposition phase might be compared with a regression prediction in which constituency, state, region, and leadership variables were the predictors. The communications phase might be compared with a regression in which the votes of a subset of the legislators with intermediate predispositions were predicted from the average predispositions of other sets of legislators whose votes had previously been determined, such as those in a particular party, party leaders, committee members, or state delegations.

The central point of this criticism is that methods like regression analysis (including probit analysis) permit a new estimate of the contribution of each predictor for the data at hand, or a new test whether the previously estimated contribution of a variable holds for these data. Even if methods of this type should be impracticable for a given model, it still seems incumbent on the authors of such a model to ask whether the parameters assumed are the best, or whether in fact the variables introduced are all really necessary. A hint of this difficulty is given by the results of the "just party" model, presented by the authors: for the federal role and foreign affairs votes studied, the proportion of votes accounted for correctly was 78 and 83 per cent respectively. The authors do show that certain features of the model make positive, negligible, or negative contributions, but these are gross features; these demonstrations do not provide new estimates of the numerical values of dozens of individual parameters, or tests as to whether these parameters are necessary.

While the cumulation of plausible hypotheses from the literature, together with plausible values of parameters, may give the simulator a feeling that he has approximated the complex reality of congressional decision making, it violates important canons of scientific research. It risks the perpetuation of partial or qualified findings that have appeared in the literature—including, in this case, some of my own findings that will not bear the weight placed on them. It foregoes the opportunity to test and reject such earlier findings, which may not hold under new conditions. It proliferates variables that may overlap and duplicate one another (though the authors have revealed a few instances of this type themselves). An alternative would be a model with fewer contributory variables but with a possibility of testing the contribution of each one; such summary variables might result from a preliminary combination of detailed variables, using methods such as principal component analysis.

The main value of a simulation model of this sort seems to be that it *generates* hypotheses involving fewer variables than the entire model, hypotheses that may then be tested on a more restricted scale with appropriate controls. One realm in which such hypotheses may be tested is the intractable one of "power." The process by which this model separates those congressmen presumed to have made up their minds, from those presumably influenced by their conversation partners, may provide useful clues to the measurement of power and influence. A shortcoming of some power measures based on roll-call statistics is that they have treated each legislator equally as giver and receiver of influence, and have failed to separate his predisposition from his final vote. Conceivably an extension of the reasoning in this model could measure the influence given and received by particular legislators, and provide measures of power that could be validated independently.

Thus, while this type of simulation model may provide some insights as to the implications of previous findings, it provides no rigorous replicative test of these findings and cannot therefore be regarded as a tested scientific hypothesis. It is a cumulation of particular speculative hypotheses, most of which are untested on the data at hand. It may well be a source of further testable hypotheses; but its scientific value lies in this future possibility far more than in the accomplishments presented in his book. An incidental virtue of the book, however, is that it presents extensive and organized bibliographies on simulation and on legislative behavior.—DUNCAN MACRAE, JR., *The University of Chicago.*

*Selective Service and a Changing America.* By GARY L. WAMSLEY. (Columbus: Charles E. Merrill Publishing Co., 1969. Pp. 259. $2.95, paper)

This study has two dominant themes which, to be fair to reader and author, should be reviewed separately. First, Wamsley provides us an excellent case study of administrative politics; he simultaneously informs regarding the Selective Service System and instructs regarding politics in semi-invisible administrative agencies. Second, the study is an exploration of the hypothesis that the survival of an institution is dependent on how it adapts to the political culture. In short, the book has a topical theme, the Selective Service under fire, and a theoretical focus, the necessity for social structures to be consonant with cultural values. About the theoretical issue we will have more to say shortly, but first a few remarks about the book as a case study of the Selective Service System.

Wamsley has drawn a precise and informative picture, but not overdrawn it. Although his own preferences and judgments are clear—despite frequent disclaimers—they do not much interfere with the presentation. Social scientists ignorant of the workings of the selective service system, as I was prior to reading Wamsley's study, will be rewarded by the few hours it takes to read it. And, of course, there is more than just descriptive information about a governmental agency. To briefly note just two of the points I found the most interesting. The book makes clear what criteria are used by draft boards to establish, in effect, a national manpower policy. "Board decisions rest upon middle class values, a legionnaire outlook, and upon diffuse and particularistic interaction with the registrant or his file." (p. 154). Elsewhere the author points out that the potential draftee with scientific and technical skills is most likely to avoid military service. On balance, what we learn from this investigation of selection criteria is something about the priorities of a group of men who control a particularly interesting governmental agency. The Selective Service System is rivaled only by the Bureau of Internal Revenue in its ability to cut through the protective layers between citizen and government to directly affect lives. That the decision rules of this agency have seldom been subjected to public scrutiny can only astound. A study of these decision-rules tells us much about the implicit values which the U.S. lives by. (In fact, such a study may well tell us more about the "political culture" than attitude surveys.)

Another, and related, series of observations in the study puts it firmly in the tradition of the many "demythologizing" books presently being published by American social scientists. The Selective Service System has lived by its myths (voluntarism, civilian control of the military, decentralization of decisions, an intimacy between citizen and state). First political and now scholarly attacks on these myths, coupled with a more realistic account of the actual operation of the System, have begun to make vulnerable what has for decades been one of the most invulnerable administrative agencies. The book contains much that is relevant to an understanding of an institution under attack.

However, even as a case study the book is flawed in one important regard. The struggle to explain the decline of support for the Selective Service would have been eased had the author more systematically placed the mounting attack on the draft in the context of an unpopular war. There is an air of unreality about the book, as if what is happening regarding the draft is not completely a part of what is happening in civilian-military relations in this country. In fact, if anything, the inequities of the draft spearheaded the changing mood toward the military-industrial complex. Somehow this is slighted. To be sure, the author is not ignorant of what is happening. For instance, he clearly notes how a surplus labor pool combined with increasing callups underscored the arbitrariness of the manpower policy being followed by local boards. What is missing in the book is an integration of this fact into the general analysis. But this is to anticipate the second part of the review, which is about the "general analysis."

Wamsley did not just want to write a case study. He also wanted to write about institutions and culture. To do so he commits several of the worst social science sins. He reifies, he fails to operationalize, he uses theoretical constructs as if they were empirical variables, he introduces and then ignores requisite analysis, and so forth. One quotation will provide some flavor: "It is suggested that societal imperatives can be more clearly understood if characterized as political culture. Therefore, a conscription system must always seek an equilibrium between efficiently procuring manpower to defend the society and society's demands that the procurement be done in accordance with norms and values of political culture." (15) There are many, many sentences of this type —all, to the reviewer at least, somewhat bewildering. What the author is asserting throughout is that American political culture is changing. Thus, an institution which earlier was consonant with that culture, but which has not been altered, is now in conflict with cultural values and consequently is politically vulnerable. One thing only needs to be said about this type of social analysis. "Political culture" if it be a variable at all, is a global, diffuse, and macro one. It cannot be forced to do the work of an independent variable in what is finally a causal theory. Such an attempt can only lead to reification and sentences which grow more abstract with each phrase. About political culture as an independent variable one is tempted to say, "that which explains all, explains nothing."

If one cuts through the language about "the institution's ability to achieve acceptance by relating its structure and processes to American political values," I think what the reader finds is a telling point about contemporary politics. It is a point which in its theoretical implications is more in the tradition of political sociology, with its emphasis on power and social class, than structural functional analysis, with its emphasis on societal requisites and political cultures. The Selective Service has long been under the control of the middle-class activists who control most local agencies—city councils, school boards, etc. The Selective Service therefore has been organized around the values dear to this part of the citizenry; it has allocated benefits and penalties in such a manner as to generally reward some social classes and racial groups more than others. This particular fact has become clearer to many citizens at a moment when political organization is increasing among previously disorganized citizens and when disenchantment with military policy is peaking. In short, the invisible Selective Service System managed by the paternal figure of Lt. General Hershey is now visible, thus politically vulnerable, and Hershey can no longer hide himself and his policies behind the protective shield of the invincible military. If there is a theoretical lesson to be learned about American political culture, it is not found in the obfuscating language about "structural-cultural consonance" but in the clear empirical finding that a mistake of the dimensions of Vietnam is bubbling up and will continue to do so throughout American institutions. It is this contagion effect which is the theoretically potent, if implicit, finding in Wamsley's study.—KENNETH PREWITT, *University of Chicago.*

*Riots and Rebellion: Civil Violence in the Urban Community.* EDITED BY LOUIS H. MASOTTI AND DON R. BOWEN (Beverly Hills: Sage Publications, 1968. Pp. 459. $8.50.)

This volume contains a collection of 25 articles, about half of which originally appeared in the March-April, 1968 special issue of the *American Behavioral Scientist* edited by the authors. The essays are of uneven quality and the haste of preparation often is painfully evident both in the analyses and the editorial work. However, there are some important questions raised here about the causes and consequences of the recent disturbances in our cities.

It is difficult to piece together an accurate account of the actual events of the last few summers and especially difficult to make an acceptable *post hoc* evaluation of the motives of the participants, even when they are available for interviews. As Alan Grimshaw points out in his piece on "Three Views of Urban Violence: Civil Disturbance, Racial Revolt, Class Assault," no simple explanation will do. Whatever complex of forces stimulated the events and whatever the varied motives of the participants, both moderate and militant blacks now subscribe to the racial revolt thesis and there is evidence, as Grimshaw points out, of "a growing increase in the strength of the previously attenuated solidarity between Negro middle and lower classes."

If we accept, as do the editors of this volume and most of the authors, a notion that these were primarily revolts against the system (or as Massotti and Bowen call them, examples of "civil violence . . . characterized by attacks on symbols of the political order *because* they are symbolic of that order—though in this case spontaneous and not attempts to seize state power"), then it is not much of an explanation of say that participants and their sympathizers are relatively deprived, dissatisfied or alienated. We need to know more about the nature of the deprivation and commitment to these tactics among blacks (versus more "legitimate" political behavior or even organized rebellion), so that the factors can be identified which will alleviate, cool down, or exacerbate the situation leading to unrest.

A great deal of work remains to be done on the issue of deprivation. The concept of relative deprivation assumes the existence of a reference group or base line which serves as a meaningful standard for the individual. In most studies of civil violence it is either measured very indirectly (Gurr, for example uses indices of "short-term" and "persisting" deprivation) or by means of such devices as the Cantril Self-anchoring Scale which indicates the discrepancy between an individual's definition of the best possible life for him and his actual and/or expected situation. In the latter case, the base line is clearly the person's own standards which may be determined by a host of factors such as class or race models, or expectations created by the media. If psychological deprivation plays the role assigned it by many theorists, we not only have to understand the processes which determine the mode of its political expression, but also its genesis. Both the nature of the relevant reference group and the way a person learns that he is deprived should be important determinants of the intensity and expression of deprivation. Successful revolutionary elites, in fact, recognize this fact and devote a great deal of effort to characterizing (and provoking) the enemy and members of the "oppressed" group in a way that creates and/or intensifies a sense of deprivation and guides behavior. Tactics, reference groups, social conditions, and communications networks are interacting elements in this process.

Previous research, summarized by Leonard Berkowitz in this volume, indicates that a sudden decline after steady improvement or a growing gap between rapid improvement and even more rapidly increasing expectations sets the stage for upheaval, but Berkowitz correctly points out that "a complete understanding of the violence (in America's cities), and especially the contagious spread from one city to another, requires consideration of a multiplicity of causes, all operating together." We are now beginning to accumulate good time-series psychological data which should help in refining the complex models necessary to understanding the sources (and consequences) of civil violence.

Granted that much has to be done, an indication of the possible payoff of developing complex models can be found in the chapter by Ted Gurr. The article in Masotti and Bowen's collection is an abbreviated version of Gurr's work reported in recent issues of *World Politics* and this *Review* which interested students should consult for a full explication of his position. Seven independent variables are employed to explain the magnitude of turmoil ("relatively spontaneous, unstructured, mass strife, including demonstrations, political strikes, riots, political clashes, and localized rebellions") in 114 nations from 1961-1965. Gurr uses partial correlations derived from a multiple regression using all predictors to sketch a model of the sequential causes of turmoil. He finds that

> if coercive forces are large and loyal, if few facilitative conditions are present [extremist organizations, good terrain, etc.] and most important if there is no tradition of civil violence, short-term deprivation is unlikely to lead to violence. If these intervening conditions are substantially different, however, even mild deprivation is likely to result in turmoil. [In addition,] high legitimacy is an important and independent source of low turmoil.

Applying these findings to the American situation, Gurr believes that our tradition of violence, the growth of extremist organizations, and the increasing number of black Americans who seem to regard the political system as illegitimate all suggest that "turmoil will be chronic in the near future." In addition, if racial discrimination (an element of the measure of long-term deprivation) persists, then, according to Gurr, the country is likely to experience violence regardless of the state of the intervening conditions cited above since long-term deprivation has "a major direct effect on the magnitude of tumoil." If one assumes that most blacks' psychological reaction to discrimination has changed radically—they no longer see it as inevitable and will not stand for it—and that discrimination is not about to disappear, then the implications are discouraging for the near future. If Gurr's model is basically correct, inaction should have severe long-run consequences. The political problem, of course, is that positive governmental programs often create short-term problems and have a mixed effect on legitimacy since they anger many whites because of perceived governmental favoritism toward blacks and at the same time raise expectations in the black community well beyond achievements. Faced with the prospects of competitive mobilization within the two racial communities, leaders are tempted to do nothing during their own tenure or play to the sentiments of the large minority of angry whites.

It is very important to stress the fact that Gurr's findings have implications for whites as well as blacks. Many whites, for a variety of reasons, are also questioning the legitimacy of the government, feel that they are being deprived, and are part of our culture in which violence has been fairly commonplace. One problem which political scientists interested in public policy must examine further is that the very policies which are designed to aid blacks not only foster at least short-run discontent in the black community, but stimulate backlash among whites. The tenor of this backlash is ultimately the crucial factor in today's profound crisis in American politics.—Joel D. Aberbach, *The University of Michigan.*

*Government Without Passing Laws: Congress' Nonstatutory Techniques for Appropriations Control.* By Michael W. Kirst (Chapel Hill: University of North Carolina Press, 1969, pp. 167 $5.00)

Encompassed by the constraints of developments which have recognized the need for administrative flexibility and rules which prohibit legislation in appropriations bills, how can appropriations committees and Congress nonetheless control the activities of administrative agencies? Michael Kirst has found that they have come to rely on nonstatutory techniques, primarily appropriations hearings, reports accompanying bills, and debates on the bills.

In this study Kirst raised a number of questions: 1) what are proper areas of committee control, 2) how and under what conditions are nonstatutory devices effective control techniques, 3) why are they employed, 4) are there any "dangers" inherent in their use, and 5) does the use of these devices assure the committees' meeting their proper roles? The questions constitute a mixture of normative (1, 4, 5) and explanatory (2, 3) concerns toward a particular policy.

In a somewhat syllogistic style of analysis Kirst finds it proper for appropriations committees to be involved in both broad substantive policy and administrative details of that policy. For one, they are already affecting policy by setting agency fund levels and conditions for spending. Further, legislative committees have failed, through disinterest or inability, to exercise close control over policy. Because some Congressional group must assume responsibility, it is proper that appropriations committees step in. It necessarily follows that control over administrative details is necessary to control broad policy. Committees are thus free to utilize nonstatutory techniques to control both policy and administrative detail, and any evaluation of these techniques must be in the context of their contribution to these ends. At all times, however, the committees' use of these techniques must be under the ultimate control of the parent house. This is a practical justification where one easily gains the impression that the purpose of the argument is to establish the legitimacy of an unrestricted study of the techniques, rather than a legal/constitutional inquiry. Otherwise we would expect Kirst to address himself to a development which he mentions on the next to last page of the book—that legislative committees are indeed demonstrating greater interest in oversight of policy through the requirement of annual authorizations (over 35 percent of the money in the President's 1967 budget was subject to annual authorization). Does this development reduce or eliminate the propriety of appropriations committees' involvement in substantive policy?

Kirst attempts to answer the remaining questions by drawing on earlier writers (e.g., MacMahon, Wallace, Huzar), by analyzing the printed Congressional documents relating to appropriations bills (hearings, reports, and *Congressional Record* debates), and from interviews with some committee members (6) and clerks and departmental budget officers.

In a style similar to Wildavsky's and Fenno's studies of appropriations politics (eclectically utilizing quotes from the printed record to illustrate or establish his point) Kirst demonstrates the delicateness and subtleties involved in nonstatutory control, both during hearings and in reports, where carefully developed language serves as a code guide to agency behavior. Agencies must learn this code and calculate the risks in failing to adhere.

Committees assure congruence between their language and agency behavior by asking questions at next year's hearing, investigating, inserting legal restrictions in the bill, and cutting budgets. The effectiveness of these sanctions depends upon internal agreement among subcommittee members, the political clout of the subcommittee, agreement by other Congressional sets, and whether the control attempt conflicts with broad administration policy.

Appropriations committees resort to nonstatutory techniques to overcome and satisfy the constraints cited at the outset of the review. Because they are not legally binding it affords the committee the opportunity to express its wrath while making it possible for the agency to disregard the message if later circumstances justify. Mutual accommodation harms no one. The devices also make possible committee initiative in policy areas without incurring points of order and questions during floor debates. Does all this not serve to allow appropriations committees to operate unchecked? No, for the entire appropriations process serves to confirm or dilute a control attempt made at one point. An "instruction" by the house committee, if not repeated by the Senate or conference report, or if countered by sufficient commentary during floor debate, could lose its mandatory character in the eyes of the agency (if not in the eyes of the originator). Kirst's overall evaluation is that the legislative body has the opportunity and has demonstrated both the willingness and capacity to control its committee. In light of this guarantee Kirst concludes that nonstatutory techniques should not be surrendered as long as they continue to serve to meet the proper role of the committee.

The principal problem of the book is that Kirst attempted to simultaneously serve both evaluative and explanatory objectives in a brief space using a broad general descriptive research and reporting approach that served neither objective adequately nor sufficiently. We are not provided with a systematic empirically arrived basis for his evaluations. More hard data are necessary to support conclusions from which policy evaluations can be made. First steps must be taken, and the problem of the book is also its virtue. Kirst has taken a successful first step in this area. Our understanding of the use of nonstatutory techniques has been deepened by this study.

To this reviewer then, the contribution of the book rests not in its normative treatment for Kirst's policy analysis is not very powerful nor convincing, but in those chapters where he describes the processes and behavior related to the use of the techniques. It is here where students of the appropriations process and executive-legislative relations will find a useful addition to the literature. Further, while Kirst appears to eschew systematic analysis of quantified data himself (the only table in the book and his commentary may provide evidence why), the possibilities for pursuing interesting theoretical and policy questions related to nonstatutory techniques in such a manner are evident. It is hoped that a number of readers will peruse this volume to explore the comparative research possibilities latent within. Following such research an evaluative study of the nonstatutory techniques policy could be more convincing to political scientists.

The reader should also be altered to some disquieting features of the book resulting in part from editing carelessness in updating and expanding a dissertation for publication, and in a tendency to repeat points and footnotes which creates a "deja vu" feeling. This tendency can be attributed to the organization of the book rather than to any attempt to stretch the research input.—JAMES E. JERNBERG, *University of Minnesota.*

*The Negro in Third Party Politics*, BY HANES WALTON, JR. (Philadelphia: Dorrance & Company, 1969. Pp. 123. $4.50.)

Professor Walton's study of *The Negro in Third Party Politics* fills a very significant and irritating void in the literature of American political parties. Although, as he readily admits, there have been two books published in the general area, neither of these go beyond the Negro and the Communist Party.

*The Negro in Third Party Politics* is a brief but fact-filled historical account of third-party activities affecting the status of the American Negro. It is divided into two major periods—pre- and post-Civil War—with major emphasis upon the post-Civil War period since it was only after the Civil War that the potential "Negro vote" began to appeal to the major and minor parties. In his discussions of the pre-Civil War period, Professor Walton concentrates on the slavery policies of the parties and attempts to assess the impact of these parties upon the abolition of slavery. The anti-slavery parties, (the Liberty Party, the Free Soil, the National Liberty Party, the Political Abolition Party), he concludes, aided in the eventual abolition of slavery by publicizing the evils of slavery. The pro-slavery parties (the Nullification Party, Southern Rights Party, the Native American Party, the Know-Nothing Party, the Constitutional Union Party) inadvertantly aided the anti-slavery forces by making slavery "a primary political issue . . ." and thereby sharpening "the factional rivalry in both major parties over it."

The post-Civil War section is devoted to the policies of third parties as they relate to the achievement of "full citizenship" by Negroes. The theme which seems to emerge from this section is that the Negro vote, particularly since the end of the Civil War, has been almost constantly misused. It is not clear whether the author intended to project this theme. Nevertheless, the data offered clearly demonstrate that all too often the Negro has been used as a pawn in the game of power politics.

Many examples of the misuse of the Negro's voting strength occurred during the period following the Civil War when Republicans, because of fear of the consequences of restoring home rule to the South, guided the Negro vote. As the author points out, "many devices were used by northern soldiers, agents of the Freedman's Bureau, and the Union League, to see that the Negro voted properly, i.e., for the Republican Party."

A more recent example of attempts to misuse the Negro can be taken from the experiences of the Union Party of the 1930's which "was a coalition of Father Charles Coughlin's 'Social Justice' followers, and the followers of Senator Huey Long's 'Share the Wealth' movement. . . ." Prior to the 1936 elections the Union Party openly sought the support of Negro voters by calling for an end to discrimination against anyone because of color. It was obvious that the Union Party was attempting to deceive Negro voters. The attempted deception was so obvious, as Professor Walton points out, that the "N.A.A.C.P., in an editorial, urged Negroes to 'beware of Coughlin and anyone he supports.'" In case after case the data suggest that in many instances third parties, like the major parties, have used the Negro vote for their own selfish gains without regard for the welfare of Negroes.

In addition to providing an excellent history of the Negro and third party politics, Professor Walton has assembled an admirable political history of the American Negro in that he has gone beyond merely stating the various positions of the minor parties as these affect America's most oppressed minority. He has additionally recounted in some detail many political episodes directly affecting the Negro. One example of these political episodes is Professor Walton's interesting discussion of Theodore Roosevelt and the question of seating Negro delegates from the North and the South at the 1912 National Convention of the Progressive Party.

In spite of the many positive features of this study, it does suffer from one major shortcoming—the apparent lack of information about contemporary "all-Black" third parties. It may well be that these parties will become the vehicles through which the Negro will make felt his newly-found political power. In view of this possibility, any study of the Negro and third party politics should place considerable emphasis upon "all-Black" third parties.

*The Negro in Third Party Politics* is a very readable study of a most interesting topic. Every serious student of American political parties must become acquainted with this work.—EDWARD JACKSON, *Southern University.*

*Planning and Politics in the Metropolis.* BY DAVID C. RANNEY. (Columbus: Charles E. Merrill, 1969. Pp. 179. $2.25.)

David Ranney has done students of urban politics a service by synthesizing much of what we know about the relationships between physical planning and city politics. His book converts much of the best knowledge garnered from modern political science and planning theory, at both the city and metropolitan levels, into an integrated text. That the task has been done by a planner and not a political scientist is auspicious since up to the present, concern for the politics of planning by planners themselves has been rare.

Ranney discusses the variables current research indicates are critically relevant to planning action, in terms of Banfield and Wilson's notion of the city as having service and conflict management functions. What the planner recommends is categorized as the "service" aspect of planning policy decisions. This is effected by the attitudes of the planner, the type of planning agency a city has, the nature of federal policies and metropolitan jurisdictional divisions (only the Department of Housing and Urban Development loves the metropolitan planner); and the kind of political system the planner confronts. How the political system reacts to the planner's proposals (the "conflict management" aspect of planning) depends on the kinds of planning policies pursued, the values the community holds about the functions of government, the role of the planner, the distribution of power among various segments of the community (less important in the long run than is generally believed), and the socio-economic characteristics of the city.

Ranney also describes what planners normally do and touches base on new developments in the planning profession which are occurring as it begins to lose confidence in its ability to make the environment better for urban dwellers. He traces the movement from urban physical to social and policy planning and more recently, to advocacy planning.

There is more specificity about factors affecting planning than about the outcome whose variation is examined. This is because a thesis central to the book is that planning is not an end state but a process. While Ranney describes the activities in which planners engage, he believes it is incorrect to regard planning as producing a master plan or capital improvement program. Planning is instead the series of actions and reactions occurring from the time a planner makes a set of recommendations to the time when political actors determine what to do about these, plus feedback.

Ranney offers hypotheses about how each category of independent variables affects the service and political functioning of planning. The weakest discussion is that on the impact of socio-economic factors. Ranney says environment influences planning but is unable to specify how. Is this because of a dearth of studies? Or is it that planning actually does operate in a partial environmental vacuum? Other categories are richer, and simultaneously, more open to challenge. For example, Ranney says that great political participation in planning means a less cheerful prospect for the original plan. This reflects the notion that the more citizens participate in local affairs, the greater the chance of conflict and hence the lower the level of performance. And, Ranney adds that if a community is made up of groups which subscribe to a "public-regarding" ethos, planning activities will have a better chance of achieving their aims than where "private-regarding" views dominate. This reflects the view that planning serves a middle class definition of the public interest. Recent research indicates that neither of the hypothesized relationships holds without qualification in other policy areas: indeed, the more groups and actors participating in current decisions, the higher the level of outputs sometimes observed.

The possibilities for argument over explicit hypotheses are an indication of the strengths of the text. It is where such argument is foreclosed that its weaknesses lie. Ranney gives us statements about relationships within categories of variables but among categories he has given up the good fight. Yet it is in the interaction among the variables that the most compelling issues now lie. If a primary resource of the planner is his technical expertise and expertise needs to be linked to action, what costs are incurred when the technician plays politician? Given that planning in different territorial subsystems is a different process, in which is role more important than organization, and in which is community type most significant in predicting outcomes? And, is there any combination of variables which allows us to link city planning to changes in the quality of urban society in the current era of community revolution?

In part it is Ranney's conviction that planning is a process and not an output measurable by expenditure indicators and completed plans, in part his sensitivity to the diversity of communities engaged in planning, and in part the state of the source literature, that makes him hesitant to weight variables or enumerate interaction rules. This position is certainly intellectually respectable. But I for one would like to know how Ranney, who has read all there is to read about city planning and its politics, guesses particular causes act in producing particular consequences, and evaluates the work on planning that has been done.

It is useful to have the variables affecting city planning enumerated and explored. But the effort runs the risk of committing that great crime—irrelevance—when it stops short of discriminating among variables or asking if existing definitions of and research on planning are not marginal to the conditions of urban life in 1970. Of course, the writer who walks out on a limb well beyond existing studies runs the risk of being called irresponsible. The dilemma is Ranney's, but hardly his alone.—FRANCINE N. RABINOVITZ, *University of California, Los Angeles.*

*Education and Politics: The Developing Relationship Between Elementary-Secondary and Higher Education.* BY MICHAEL D. USDAN, DAVID W. MINAR, AND EMANUEL HURWITZ, JR., (New York: Teachers College Press, Columbia University, 1969. Pp. 190. $6.50.)

Educational goods and services are one of the more socially significant as well as expensive outputs of the political systems of modern societies. But until very recently the politics of education was a virtually unknown domain of political scholarship and research. As a result, we now know relatively little about the politics of education in a period of history when the political significance of education is rapidly expanding. Happily, a growing number of political scientists as well as professional educators are now turning their attention to the study of educational politics. In a few cases this is a collaborative effort that blends the particular interests and skills of the educator and political scientists. Such is the case with *Education and State Politics* by Michael Usdan, David Minar and Emanuel Hurwitz, Jr. Minar is a political scientist with a long-time scholarly interest, as well as some practical experience, in the politics of education. Usdan and Hurwitz are professional educators who combine an experience in several kinds of educational roles with considerable background in the academic study of politics. The result of this collaboration is a short book which should prove useful to both the educational politicians and the academic scholar seeking to understand changing character of the politics of American education.

The book, as its sub-title indicates, focuses upon the relationship between elementary and secondary education on the one hand and higher education on the other. It begins by noting that historically the American education system as a system through which "the country's resources are allocated and organized for education" has been highly fragmented. Not only has the system been fragmented across levels of government and between different units at each level, it is also fragmented in the division that historically has separated elementary-secondary and higher education. This boundary, which was once accepted as so natural as to warrant little comment, is now being eroded as a result of a variety of socio-cultural changes, including the expanding cost of education, growing skepticism about the effectiveness of many established educational forms and procedures including the traditional divisions between grade levels, and the growth of demands for new educational services such as programs in vocational and adult education that overlap the traditional provinces of the schools and the universities and colleges. There is now emerging, the authors argue, a slowly integrating overall educational system in which the traditionally separate and partially isolated levels of American education are becoming components of a single system in which traditional relationships of mutual isolation between the schools and higher education are giving way to relationships of collaboration and conflict. This book is an effort to describe and to evaluate these emergent relationships in order to develop answers to three general categories of questions: (1) What forms are the changing relationships between elementary-secondary and higher education assuming? (2) What processes are affecting the relationships? (3) Why do these forms and processes differ as they do from state to state?

The authors attempt to develop answers to these questions by doing two things. First, using data obtained from interviews with relevant decision-makers, the analysis of documents, and reading of press accounts, they construct "profiles" of the relationship of elementary-secondary and higher education in twelve selected states. These descriptive profiles, which evidence an admirable degree of comparability, seek to describe in abbreviated form the organization of education in each of the twelve states, the critical issues involved in inter-level relationships, and projections about the future of these relationships.

Secondly, using these profiles and the larger sets of data from which the profiles were constructed the authors develop a comparative analysis of what they term to be "some major elements" in interrelationships of pre-collegiate and collegiate education. This analysis is focussed upon patterns of inter-level conflict and cooperation. Cross-state differences are noted and analyzed in terms of a range of factors which might partially account for or explain variable patterns of conflict and collaboration. These include the changing structure or character of major interests in educational politics, e.g., the growth of militant teacher organizations; the formal structure of educational government, e.g., elected vs. appointed state board and/or superintendents; the structure of state political systems; the character of the issues involved in inter-level relationships; and the socio-economic environment of state educational systems.

The book concludes with a brief outline of policy recommendations. Given the fact that the schools on the one hand and universities and colleges on the other are no longer isolated organizations but rather components within a single system of educational politics, the future of inter-level relations is one of either intensified conflict over scarce resources and alternative educational policies or one of expanded inter-level cooperation. The authors note that while they are by no means committed to an ideology of conflict-free politics, on the whole they are "pushed toward the conclusion that inter-level coordination in education is a desirable, if not essential, step" if the states are to develop a capacity for rational planning congruent with the educational needs of the times. They argue that expanded cooperation is not likely to come about as a result of "natural" accomodation among traditional interest groups in education, nor are governors or legislatures currently equipped to bring it about. There is a need, they argue, for the creation of "new overall coordinating mechanisms."

In conclusion it should be noted, as the authors freely acknowledge, that *Education and State Politics* is not a complete study, but rather, as they put it, a "pilot study," an attempt at "analysis of a subject that deserves much more attention." This is true, but the work is nonetheless a very valuable contribution to the gradually expanding literature on the politics of education. Beyond this, it is an example of scholarship that seeks to use social science in order to describe, analyze, and evaluate a significant social problem and then present the resulting product in a literate way.—LEE F. ANDERSON, *Northwestern University.*

*The President and the Management of National Security.* EDITED BY KEITH C. CLARK AND LAURENCE J. LEGERE (New York: Frederick A. Praeger, 1969. A Report by the Institute for Defense Analyses. Pp. 274. $6.95.)

Scarcely two decades have passed since the Congress expressed its intent, in the National Security Act of 1947, "to provide a comprehensive program for the future security of the United States; [and] to provide for the establishment of integrated policies and procedures for the departments, agencies, and functions of the Government relating to the national security. . . ." Both the internal and the external settings have changed enormously, but in the wake of that change, three key creations of the Act, the National Security Council, the Central Intelligence Agency, and the Department of Defense, have come to play significant roles in the decision-making process related to national security.

The present study, sponsored by the Institute for Defense Analyses, omits the background history and developments leading up to the National Security Act of 1947. It concentrates on the National Security Council, proceeding directly from a premise that "if there were now no NSC, something very much like it would have to be created." Far from focusing exclusively on organizational structure and mechanisms, however, it ranges over the broader ramifications of the deliberate effort to create arrangements for policy planning. A major effort is made to point out options, within existing legislation and institutions, that might improve the national security process.

Because each President has used the NSC in his own way, its actual role and responsibilities have varied widely. Nevertheless, a clearly discernable contrast between the "formalized-codification" approach of the Eisenhower administration and the pragmatic or *ad hoc* approach of the Kennedy and Johnson administrations provides the underlying theme throughout the report. Since neither administration adopted one process to the exclusion of the other, and since outward appearances can be most deceiving, the several authors attempt to get behind the more obvious differences in presidential style by drawing on interviews (not cited directly) with nearly one hundred government officials who hold or once held, key positions in the White House or in main departments and agencies involved in national security affairs. A conscious effort is made *not* to evaluate substantive performance. Instead, national security policy is treated as something like a management tool; concentration falls on the "process implications" and the relative effectiveness of each method.

Probably the most authoritative and comprehensive discussion of the national security process yet printed—and certainly the most up-to-date—this is a timely study in a number of respects. At the beginning of any new administration there is always a good deal of speculation as to the direction it may take and the dominant forces that may emerge within it. During his presidential campaign, Mr. Nixon stated that he intended to restore the NSC to its preeminent role in national security planning. He also attributed "most of our serious reverses abroad" to the Kennedy and Johnson administrations' "inability or disinclination" to make effective use of President Eisenhower's National Security Council format. President Nixon's frequent references to meetings of the NSC (and the apparent frequency of the meetings themselves), his sanction for assembling the largest and most elaborately structured NSC staff in history, and his appointment of Henry M. Kissinger as Special Assistant for National Security Affairs all indicate a significant role for the NSC, at least for the next three years. Is this an explicit reversal of the Kennedy-Johnson approach to policy formation? Is it something more than a reversion to Eisenhower's techniques? Does it represent, through rescinding NSAM 341, a rejection of the former's effort to strengthen the policy role of the State Department? What is the future of interdepartmental operations characterized by the Johnson administration's Senior Interdepartmental Group and the subordinate Interdepartmental Regional Groups (SIG/IRG)?

Answers to these and similar questions will fascinate students of national security policy formation for years to come. Mr. Nixon was very firm in stating, during his campaign: "This [National Security] Council was established by law to integrate our diplomatic, military and economic policies. It was our assurance that America would not aimlessly drift in world affairs but would control and direct their course." Reviving or redirecting the influence of the NSC, however, is but a beginning step. There has been speculation that Mr. Kissinger may be more a staff assistant and less a policy maker than his two predecessors, Bundy and Rostow. It has been reported that Mr. Nixon will reestablish the NSC's Planning Board and Operations Coordinating Board (OCB), and that he will reemphasize the extension of planning through the State Department's almost moribund Policy Planning Council. While it seems likely that there will be less emphasis on the "proliferation of papers" that characterized the Eisenhower years, and less of the free-wheeling decision-making by White House assistants, the Report draws attention to more fundamental "pressure points" which must also be considered: the general eclipse of the State Department and implications of a White House-centered system for national security; the changes in the Department of Defense and the impact of the "McNamara Revolution"; and the constant search for basic policy guidelines that will not unduly circumscribe the president's flexibility of action. In the last analysis, it will be the interplay of personalities and mechanisms that determines the over-all character of the Nixon administration's approach to national security matters, and in this context, the authors' caveat bears repeating: "The force of personality of the President and his principal advisers will have a more important influence on how major decisions are reached than will the organizational procedures that have been set forth."

In the IDA Report, we are given a valuable framework within which to consider the trend toward a national security system. Unlike many efforts involving a number of authors, the quality and style of the various sections is of consistently high calibre. The effort to achieve objectivity has created certain minor problems: discussing the subject as an abstraction (without getting into substantive matters) has produced a sense of bland detachment; too many issues are resolved into apparent dichotomies and then bridged by a catch-all "middle range of choices" category; finally, the text suffers from a certain homogenized quality—it is a sober, methodical presentation which leaves one longing in vain for an element of strong conviction, approval, outrage, or even subtle humor. At any rate, the issues are presented with clarity and precision. The elegance and style can be found among the references cited in the excellent bibliography.—Herbert F. Steeper, *University of Massachusetts.*

*Traffic and the Police: Variations in Law-Enforcement Policy.* By John A. Gardiner. (Cambridge: Harvard University Press, 1969. Pp. 176. $6.00.)

Social scientists are beginning to give systematic attention to the police as critical actors in the distribution of values. After years of lamentable oversight, many social scientists seem to be following the lead of journalists, novelists, civil liberties attorneys, and assorted others who have worried over the importance of the police in the urban world. It may be that the various presidential commissions have acted as important catalysts for this new, or at least greatly expanded inquiry. In commissioning papers, hiring consultants, and so on, the presidential commissions may have turned some academic priorities around, even if they have failed to turn the nation's priorities around. Perhaps one should take what comfort one can.

While much of the recent literature by political scientists, about police, has been theoretical speculation, often concerning critical policy issues, empirical research is beginning to appear. One early product of this newer scholarly interest in the police (not apparently related to any presidential commission incentive) is John A. Gardiner's *Traffic and the Police.* This valuable little book is an intensive examination of police traffic-ticketing behavior in four middle size Massachusetts cities, supplemented with a survey of state and national data which presents corroborating evidence. The study has the principal objective of accounting for the widely varying patterns of enforcement of traffic laws. In particular, Gardiner tries to examine the degree to which police ticketing practices are explained by police departmental variation (policy of chiefs, existence of traffic divisions, departmental norms and incentives to write tickets, etc.), and the degree to which they are explained by demographic, political, or judicial characteristics of the city (court disposition of tickets, local accident rates, standard demographic measures). Gardiner's evidence, substantial but perhaps incomplete, is that police departmental variation, and particularly the priorities of the police chief, are much more important than anything else, in accounting for the variation in enforcement.

Writing this review in Chicago, it is superficially easy to short-change a study of police traffic ticketing behavior as of relatively minor importance compared to other researchable questions about the police a political scientist might examine. Gardiner's research appendix acknowledges the importance of data availability in the choice of his subject, and he sticks quite close to his data. Yet, police ticketing behavior may be a clue to other police behavior, as the author notes. Surely in efforts to understand the parameters of police discretion and the nature of street corner justice, research into police norms generally, and the responsiveness of policemen to central direction, is critical. But many of the useful insights in the book must be mined, for the author did not elaborate on such questions himself. Gardiner's analysis of the inability of public reaction to influence policy, due to a total lack of public knowledge about what police traffic enforcement policy might be, plus his discussion of the inequalities in traffic law enforcement, is of substantial importance.

The combination of intensive description of police behavior in four cities, with a supplemental survey analysis, is no longer unique in its combination, but it is still notable, both as a multiple method enrichment of the findings, and as a more broadly enlightening format for presenting a case. The particular combination effort in this book is not without problems. The four Massachusetts cities intensively studied are Cambridge, Waltham, Lynn, and Malden. Cambridge and Waltham are the highest ticketing cities in the Boston area, while Lynn and Malden are the lowest. This is a useful finding, incidentally, for traveling academics. Critical to the whole analysis is a presumption that the incidence of traffic violations is a constant, across cities, so that variation in ticketing is a product of varying police behavior. This presumption, as Gardiner describes it, seems plausible, despite conventional wisdom (1) about who commit traffic violations (the young), and (2) who disproportionately live in the high ticket university towns of Cambridge and Waltham (the young). Gardiner rests his presumption on the substantial variation in traffic law enforcement within cities, over time. This is plausible, but the presumption is not substantially tested, even though indirect indicators of incidence seem possible. One can agree that wide variation in traffic tickets per thousand motor vehicles clearly shows that ticketing practices are not *solely* a reflection of variance in the commission of traffic offenses, but still suspect that it may be partially such a reflection.

A difficulty with the study, not entirely handled, is the perennial problem of generalizing from a small, non-randomly selected set of cases—four Massachusetts middle size cities—to a wider population. The analysis of state and nation-wide questionnaire returns, plus the secondary analysis of other national data, does buttress the case findings. But some of the key traffic ticket practices in Massachusetts seem so exotic and unlike practices in most areas of the United States that they seem likely to be the product of a particular state or regional political culture. I doubt if the semi-legitimate, and near universal "fixing" of tickets by Massachusetts police chiefs has many parallels. The benefits of doing comparative study within a single state—accessibility, similarity of formal organization, similar traffic laws, etc.—still leave problems in this study. Note that the survey data were not used to test hypotheses originally generated from the comparative study of four cities. The hypotheses were tested in the four cities, and this analysis was partially broadened and reexamined in the survey analysis. So the problem of the generality of the four city findings is important.

The relative ease of access to police chiefs and policemen, plus the high response of the national survey of police chiefs to a mail questionnaire (75%), indicates that social research into police activities is quite possible. The secondary use of state and national traffic data also suggests that such sources can provide much of use for social science endeavors to measure and account for police behavior.

Gardiner has marked out important questions, and provided valuable leads. His conclusions seem tenuous and rather narrow. With the obvious centrality of the police to the contemporary urban crisis, I hope the author, and many others, will pursue these matters.—H. PAUL FRIESEMA, *Northwestern University*.

*Employer-Employee Relations in Council-Manager Cities*. BY WINSTON W. CROUCH. (Washington: The International City Managers' Association, 1968. Pp. 137.)

Work slowdowns, mass sick calls and strikes by public employees are now occurring with a frequency thought impossible a few years ago. Sanitation workers, firemen, policemen, teachers, transit workers, nurses and social workers have all recently articulated grievances through direct action. Less publicized but more important over the long run is the organization of public employees into voluntary associations and unions. Winston Crouch has examined the present development of and attitudes toward municipal employee organizations in a selected group of council-manager cities.

Most of the data presented in the study were collected through a mail-back questionnaire sent to the total universe of city managers serving cities of 10,000 or more people (N = 914). Replies from 623 city managers (68%) were included in the analysis. The questionnaire was designed to elicit information about the following: (1) an inventory of unions and local associations; (2) the city manager's perception of community attitudes toward the organization of municipal employees; (3) officially expressed local policies regarding city employees' political and organizational activities; (4) city management practices relative to employee organizations; and (5) the manager's opinions about employee organizations.

Analysis revealed that metropolitan cities, rather than suburbs and independent cities, and the New England and East North Central areas were the mostly heavily organized. The International Association of Fire Fighters, the Fraternal Order of Police, and the American Federation of State, County, and Municipal Employees were the associations that enjoyed the greatest success in organizing municipal employees. Only 5% of the cities had more than three associations organizing their work force while 39% of the cities had no employee organizations at all.

Two major impressions conveyed to the reader by the book are that employer-employee relations have not been an issue of major importance in the past and municipal officials have reserved judgment about the role and contributions of employee organizations. Crouch reported that two-thirds of the council-manager cities had no formal policy regarding employees joining employee organizations. In one-fourth of the cities having employee organizations no organizational spokesman was involved in discussing with management such basic matters as wages and salaries, only 17% maintained signed agreements with employee organizations, paid professionals functioned as representatives in 10% of the cities, the union shop was accepted in 5%, and arbitration was used sparingly.

City councilmen, city managers and the community at-large were for the most part neutral in their attitudes toward employee organizations. City managers in over 50% of the cities reported that councilmen had given no clear indication of attitude toward employee associations. The managers had no unified attitude toward employee groups or to their role in the administrative process. Aside from organized labor relatively few community groups seemed to speak consistently (pro or con) on the subject.

The major observations made by Crouch were: relatively little organization had taken place among municipal employees aside from police and firemen; council-manager cities had done little to regularize procedures for dealing with employee groups; few cities have had serious work disruptions but the number was rising; community interest groups, other than organized labor, had shown general indifference to employee organizations; and city councils and city managers had not formed a definite judgment about employee organizations.

However, the research strategy that produced these findings should be viewed critically. Even though city managers are centrally located in the administrative process, asking them to report on the attitudes of councilmen and community organizations has some serious shortcomings. The spate of community power structure studies over the past decade and a half certainly demonstrates that to whom you address your question largely predetermines your answer. Would the same observations about employer-employee relations have resulted if councilmen, labor leaders, and community activists had been included in the survey? Certainly it is not surprising to find city managers opting for neutral type responses when asked to comment on the opinions and attitudes of others.

A few other minor points should be noted about the study. First, in several sections of the book relationships were referred to as being statistically significant. However, the reader is not informed as to the statistical treatment afforded the data nor the probability level accepted as representing significance. The reader is also due an explanation as to the reason any test of significance is used since the data were drawn from the total universe of council-manager cities and not from a sample. Second, the absence of a summary chapter in the book is unfortunate. The various lines of inquiry pursued were not woven together in any kind of final summing-up, thus necessitating the reader to do some of the author's work.

Overall, the monograph makes a needed addition to our knowledge of an issue-area of local government that will most assuredly grow in importance. As other researchers initiate new inquiries into municipal employer-employee relations this study will inevitably become a base point against which new insights and findings can be measured and compared.—Schley R. Lyons, *The University of North Carolina at Charlotte.*

## COMPARATIVE GOVERNMENT AND POLITICS

*Modernization by Design: Social Change in the Twentieth Century.* By Chandler Morse, Douglas E. Ashford, Frederick T. Bent, William H. Friedland, John W. Lewis, David B. Macklin. (Ithaca: Cornell University Press, 1969. Pp. 426. $11.50.)

A volume promising cross-disciplinary studies by representatives from several social sciences on a common topic such as modernization is typically a euphemism for another loose-jointed non-book, something one sees all too often. This collection is decidedly not of that class. It represents an important effort on the part of scholars from the fields of political science, sociology, economics, and social psychology to focus on problems of the modernization process facing "late modernizers," i.e., twentieth century efforts by new nations to ". . . augment their wealth-creating power . . ." Of course, the product is not as uniform or consistent as the work of a single author might be, but the results clearly justify the hazards of cross-disciplinary work.

Four areas of interest to political scientists and political development specialists are touched on. First, the essays provide useful discussions of bodies of literature relevant to the study of modernization, some familiar and others relatively unexplored. Specifically, the essays examine social parameters restraining leadership in the modernization process, attitude change, institutional change in terms of role proliferation and differentiation, the role of public administration, interaction analysis, and the relationship between economic and social analysis. Second, the chapters illustrate the utility of working at all levels of social analysis. Ashford treats the individual unit of analysis focusing on how individuals come to perceive new values and relate them to institutional change. Macklin, building an interaction model joining the individual and social levels of analysis, argues cogently that modernization entails significant changes in institutions, but that these changes depend in turn on changes in the psychological dimension. Freidland traces the role proliferation and increasing formalization of institutional arrangements that occur as modernization progresses. Lewis stresses the importance of leadership, but in terms of the constraints imposed by the social forces which structure, at least in part, the responses open to modernizing political elites. Morse, after a perceptive critical review of classical and neoclassical approaches to economic growth, argues (and I believe him correct) that ". . . economics as we know it in the West is not concerned with the problems of basic institutional change and has nothing to say about them." He begins the task of rectifying this omission by relating economics to all possible aspects of institutional and revolutionary change. Third, as promised in the introduction, the essays are concerned substantively with the crucial distinguishing characteristics of premodern and modern societies and the ways in which premodern societies were transformed into modern societies. Finally, the authors are not content with theoretical discussions, each chapter includes a section on the policy relevance of their perspective for development strategy worth serious consideration from foundation and government people concerned with development.

Although a reading of any or all of the essays would be very productive, the long chapter by Morse stands out as an extraordinary model of creative yet systematic and precise writing in the nexus between economics and the rest of social science. Morse accomplishes this by being rigorous when possible and open-ended when the situation calls for it. For instance, he critically presents aspects of "replicative" (premodern), classical, neoclassical, and progressive economic growth as they relate to basic institutional change (the heart of modernization for Morse) in a succinct but elegant manner using symbolic notation where useful. Next, he examines institutional change in general viewing institutions as ". . . ideas, concepts, symbols, rules, statuses, relationships, and so on . . ." Essentially, institutions may be considered the parameters of social action. He examines the modernization implications of institutional change at the cultural, the organizational, and individual psychological levels. Radical or revolutionary internal change emerges as the crucial determinant for successful modernization. Simply put, any social scientist concerned with modernization should read this essay.

The authors, particularly Lewis and Morse, ignore implications of their own analytic sections when making their development strategy remarks. The evidence accumulated by the authors points to the massive structural difficulties facing elites attempting to modernize now. Lewis identifies the problem as that of diverting leadership structures in modernizing countries from their initial highly ideological thrust to more "rational" involvement in productive processes associated with economic growth. Lewis observes that ideology provides an initial substitute for social cohesion but should be subsequently moderated so that leaders can "think contextually" in order to engage successfully in the development process. Lewis correctly identifies the dangers of ideological emphasis for the long term development process. However, the weight ideology must bear as a substitute for social cohesion may be of a higher order than Lewis admits. Lewis implies that the Chinese Communist leadership may be faulted for not softening their ideological thrust and thus may be blamed for the large internal disruptions in the 1960's. Yet it may well be all the Chinese elites can do to keep the ideological network, the substitute for social cohesion, and hence the social system in being given the social structural stresses brought about by the impact of modernization. Morse argues the case for sustained revolutionary leadership to bring about the institutional change necessary for successful modernization. Again, his own analysis equally implies that cycles of political leadership instability will proliferate as men ineffectually grapple with social forces beyond their control. Of course, this question involves theoretical assumptions about the role of political leadership and few of us are willing to argue that social forces such as those accurately described in this volume render men impotent. However, evidence continues to mount that the intensity which telescopes factors associated with the modernization process provides current political leaders with difficulties of a much greater scope than we have been thus far willing to admit.—ROGER W. BENJAMIN, *University of Minnesota.*

*Partiti politici e strutture sociali in Italia.* EDITED BY MATTEI DOGAN AND ORAZIO MARIA PETRACCA. With a preface by Alberto Spreafico. (Milano: Edizioni di Communità, 1968. Pp. 660. L. 5,800.)

An increasing interest in Italian politics is reflected in a number of excellent publications in English ranging from John Cammett's study of Gramsci through various studies of political behavior—Barnes, Blackmer, Di Renzo, Evans, La Palombara, Tarrow—to works dealing with communities in the process of change—Cornelisen, Lopreato, Paulson.

Two major themes emerge from these writings. On the one hand the theme of underdevelopment and development as played out in Gramsci's central concern with the *Mezzogiorno,* in Tarrow's focus upon level of development and party strategy, and in Lopreato's attention to the transformation of Southern Italy from a peasant to an urban society.

The second theme, reflecting perhaps an American over-concern with left-wing parties has been the emphasis given to the study of Italian communism.

It is against the background of these overriding themes, development and communism, that the present work rings its changes. While the book suffers from the same kind of eclecticism that characterized an earlier monumental study by Spreafico and La Palombara, *Elezione e comportamento politico in Italia,* it does provide the student of Italian politics with a series of detailed studies which compliment the work done by non-Italian scholars. In this sense it should be considered alongside other studies such as the publications of the *Istituto Carlo Cattaneo* dealing with political participation in Italy. (Cf. Vittorio Capecchi et al., *Il comportamento elettorale in Italia.* This REVIEW, 63 (June 1969) 589–590.)

The point of departure of the present study of political parties and social structure is the general election of 1963, though the intended focus is to explore the relationships between political behavior (almost exclusively expressed here through electoral data) and social structure. Spreafico, in the preface, concedes that the work does not present an organic synthesis but rather a problem orientation reflecting various methods and approaches.

The work is divided into six sections—Prolegomena, Influences, Migrations, Dynamics, Ecology, and Elites—and perhaps the most useful service this reviewer might be able to perform for the interested student of Italian politics is to summarize the hypotheses and conclusions of some of these rather uneven articles.

Perhaps the most interesting sections in this collection are those dealing with "Migrations" and "Dynamics." The former consists of a review by Gian Franco Ciaurro of the fourth phase of Italian emigration and more specifically the continental European character of this emigration. Thus between 1960–61 almost 1½ million Italians migrated to the Common Market countries, to Switzerland and Britain, while the transoceanic migration during the same period consisted of only 260 thousand. Equally impressive is the fact that 300 thousand of the Europe-bound migrants returned to Italy in 1963 to cast their votes in the national election. (An interesting sidelight here might be the use of "unobtrusive measures," i.e. increase in railway travel during electoral periods, to study migratory politics). The second aspect of the "new migration" is of course the internal migration which Ciaurro discusses in the light of the social and political integration of the Southern migrant. It is here that the study by Domenico De Masi and Giuseppe Pranzo analyzing demographic shifts in Milan as a center of immigration is particularly useful. After an imaginative and careful analysis involving such variables as demography, electoral results 1946–63, electoral dynamics of the individual parties, distribution of the parties in various zones of Milan, geographical origin of voters, socio-economic conditions prevalent in various zones of Milan, etc., the authors conclude that it is impossible to draw unequivocal causal relations about immigration and political behavior in Milan. They grudgingly acknowledge that the Southern workers seem to vote Communist because they are workers rather than because they are Southerners. A socio-economic analysis becomes more important than a cultural one. In a similar vein Ciaurro concludes that "Neither from the electoral data, nor from other elements does it seem possible to derive a precise correlation between migratory phenomena and the voting trends of political forces other than the PCI." (351)

Under the heading of "Dynamics" the articles by Corrado Barberis and Guido Corazziari, "Economic structures and electoral dynamics" and that by Mattei Dogan, "A phenomenon of political ataxia" furnish detailed material for the further exploration of the Benjamin and Kautsky hypothesis. The fundamental question raised is to what extent do electoral shifts result from economic change and how does one explain the gains made by the PCI. Dogan's conclusion: "In Italy, the needle of economic development continues to be pointed to the North star. While the migratory movement goes north, communism goes south, while the country becomes urbanized communism becomes ruralized. It thus goes against the current. Society moves in one direction, communism in the other. We are thus face to face with a phenomenon which one might call "social ataxia" borrowing from biology a concept which means the pathological non-coordination of body movements." (478) It is only regrettable that the discussion of economic development and political change is not placed into the context of the earlier writings of Lipset, Cutright, and Marsh.

The first section of the collection consists of an historical overview and the essay of Paolo Ungari, "From Center-Left to Center-Right: 1958–1963," comprises essentially a historical narrative of Italian domestic politics. The author's conclusion that: "the fundamental problem of political equilibrium had almost been solved, the task now was to demonstrate the capacity for effective constitutional politics" (46) seems in the light of recent Italian events somewhat premature. Orazio Maria Petracca, "Tactic and Strategy of electoral programs" addresses itself to an analysis of the party programs as they express the aggregating functions of Italian political parties. The author's conclusions are not surprising: the party programs of 1963 tend to assume a common physiognomy which cuts across class lines, they are in the author's words "dream books." Thus the intrinsic usefulness of this particular analysis is placed into doubt by the author himself when he concludes with a sceptical quote by Benedetto Croce placing into question the meaningfulness of party programs. The article by Alberto Spreafico on "Electoral Forecasts" seems somewhat out of place in this first historical section and might more appropriately have been incorporated in the section on Pressure Groups. This is particularly so since of the four polls cited only one comes even close to following the canons of sample survey research. Two polls are in effect competitions organized by *L'Espresso* and *Paese Sera* respectively transforming electoral prediction into a kind of *Totocalcio* game. Only the CISER poll taps voter preference, the DOXA and newspaper polls are indicative of potential electors' predictions of the coming election. Therefore, Spreafico's analysis is more concerned with perception of party configurations than with electoral behavior. In predicting or expressing preferences as to the outcome of the 1963 election the overall pattern was that the estimates of DC, PSI, and Monarchist votes were overvalued, whereas the results for Communists and Liberals were often grossly underestimated.

A second section of the work, gathering together "influences" upon the 1963 election, consists of a series of articles by Prandi on the intervention of the church hierarchy, by Bechelloni on trade union influences and by La Palombara and Gloria Pirzio Ammassari on the electoral activities of the Italian Confederation of Industry. All three essays are essentially historical in character. Prandi bases his findings on selected pastoral letters, Bechelloni eschews a sociological analysis because he claims that the necessary empirical data are missing, La Palombara and Ammassari focus on Confindustria media. Of the three interest groups, Confindustria seems to be the most enlightened reflecting an increased neo-capitalist position which recognizes, within limits, the economic and social functions of the state. In contrast the church hierarchy in no way reflects the internal evolution to the left of DC. As for the trade unions the fact that the new tendencies which emerged between 1961 and 1963 were not institutionalized, prevents the Italian unions from achieving autonomy from the parties, from the government, or from the owners.

The section on "Ecology" contains a sophisticated article by P. A. Allum on the political ecology of Naples as well as a more historically oriented article by Luciano Cavalli on social and political alignments in Genoa. It is particularly Allum's article which deserves greater attention than is possible here. A final section on elites contains articles by Giovanni Di Capua on choice of candidates and an article by Alberto Spreafico on the political composition and the social stratification of the Senate.

It is clearly impossible to do justice to fifteen articles within the compass of one review. It is regrettable that the editors did not see fit to attempt to relate the various analyses to each other and there is a frustrating lack of the authors speaking to each other. Nevertheless the work will be particularly useful to those interested in economic development and migration, as well as to that small, and passionate, band of Italophiles.—Lucian C. Marquis, *Pitzer College and Claremont Graduate School.*

*Opposition in a Dominant-Party System: A Study of the Jan Sangh, The Praja Socialist Party and the Socialist Party in Uttar Pradesh, India.* By Angela S. Burger. (Berkeley: University of California Press, 1969. Pp. 306 $8.50.)

Thanks to the Ford Foundation, the Fulbright program, the American Institute of Indian Studies, the openness of Indian society and the accessibility of Indian political processes, the first generation of scholarly output of India addressed itself substantially to all-India perspectives and affairs. The second generation, equally well endowed, would seem to consist largely of monographs dealing with state and local politics. The second generation work under review supplements the Brass analysis of Congress dominance in Uttar Pradesh (1965) by focusing its attention on the problems of building and maintaining opposition parties in a dominant party system. Mrs. Burger's book is in many ways superior to the earlier work and yet it suffers from certain similar weaknesses in approach.

The book is divided into four parts. Part one elaborates nine working hypotheses and provides context by giving a brief political history of the state. These hypotheses to be confirmed, modified or rejected fall into two sectors: those dealing with the building and maintenance of opposition parties on state and national levels and those pertinent only to the local constituency level. Mrs. Burger's most salient observations derive from her study of the latter.

An analysis of the socio-economic background and career patterns of 46 out of 48 Jan Sangh MLA's, 33 out of 38 Praja Socialist MLA's, and 22 Lohia Socialist MLA's makes up part two of the book. Parts three and four consist of six richly detailed constituency studies from which the author has abstracted her most interesting conclusions.

Though the author conducted her study from 1963 to 1964, the Indian electoral experience of the last two years seems to confirm her conclusion that opposition parties confront many of the same problems as Congress has experienced in maintaining successful local parties. "Frustration and disenchantment seem to be directed against whichever party is victorious on the local level." (275) The candidate's continued victory is in jeopardy either because local candidates cannot always alleviate the basic sources of discontent or because constituency party leadership eventually ossifies to the point that newly mobilized groups can find political expression only in yet newer—or even older—parties within the constituency or paradoxically because other candidates often seem more attractive even when incumbent candidates have not failed to obtain material benefits for the constituency.

Complicating the task of maintaining party electoral support is the problem of maintaining intraparty cohesion. The difficuties created by ideological differences, by ambiguous vertical relationships between local and state units with no fixed focal point, by the lack of a socio-economic mainstay in an extremely diverse political entity, and by the arbitrary timing of elections every five years are only partly offset by the absence of an invariable party commitment, the existence of a universal franchise, and the consequent potential for mobilizing new participants.

The problems of party and electoral maintenance are common to all political parties, but in underdeveloped countries the need for opposition has been questioned by a whole generation of nationalist leaders as being dysfunctional. Mrs. Burger argues persuasively that opposition parties in fact assist the dominant party in the basic modernizing tasks of political socialization, recruitment, interest articulation, and aggregation, thus increasing the rate of mobilization and permitting "discontent, divergent interests, and antagonisms between groups to be publicly registered, to be directed against the dominant party and against governmental activities without being directed against the system." (283)

Accurate as the data concerning the six constituencies studied may be, sound as these arguments may be on the constituency level, intriguing as it all may be on the theoretical level, the claim for general applicability is never substantiated. On the basis of these six constituencies, which are not even necessarily representative of the whole state of Uttar Pradesh, we are informed that the observations are not only applicable to all of India but to all dominant party systems. Yet if this is truly a substantial comparative study rather than an area study one wonders why the constituencies were not chosen to be reasonably representative of the state (which in itself is peculiarly complex) if not the country, or why, at the very least, the author appears to have conducted her study in a vacuum, ignoring the work of people like Nayar or Wallace in Punjab, Sisson and Shrader on Rajasthan, and a number of excellent studies done on other states. In the same way, many of the author's other conclusions (the tendency for newly mobilized groups to reject opposition parties in favor of indentification with the governing party and to be welcomed therein as a means of maintaining dominance, for instance) might also have been confirmed and strengthened by reference to a number of studies including those mentioned above. Some modest reference to complementary work in the field of Indian politics (if not world politics) might have gone far in solving the problem of the great inductive leap from specific to universal.

Unfulfilled pretentions aside, however—and I stress the point only because the temptation of grandiosity is now so seldom resisted, this book is an excellent study of an important subject. It suggests a great deal about the problems of party building and maintenance in underdeveloped areas and it seems quite authorative in so far as it concerns six constituencies in Uttar Pradesh.—STANLEY A. KOCHANEK, *The Pennsylvania State University.*

*L'Abstentionisme électoral en France.* BY ALAIN LANCELOT. (Paris: Armand Colin, Cahiers de la fondation nationale des sciences politiques 162, 1968. Pp. 229.)

After the 1960 American elections, so the story goes, a small group of political hucksters undertook to peddle an aspiring Presidential candidate for 1964 with the claim that he would attract to the polls a tidal wave of alienated citizens who had been so dissatisfied with the "me-tooism" of previous candidates that they had refused to vote at all. They supported this claim by comparing the valid votes cast for President in 1960 with the size of the voting-age population and by assuming wishfully that the difference represented a politically-homogeneous mass sympathetic silently to their candidate. The electoral disaster they produced is only the most conspicuous testament to the lack of information about and understanding of the non-voter. Political scientists have been no less remiss than practical politicians in perpetrating idle myths about non-voters.

Alain Lancelot's exhaustive study of non-voting in France comes as a welcome and illuminating departure from the usual conjecture and prejudice. Professor Lancelot has left unturned very few stones, substantive or methodological. Going back to 1815, he estimates the volume of non-voting in France and subjects to rigorous analysis the many slippery problems that arise in defining, measuring, and evaluating electoral abstention. Then, in the distinguished French tradition of Siegfried and Goguel, he spreads his hatch marks across the hexagon and discovers that with non-voting—as with nearly everything else—France is two countries, split somewhere near the Loire.

In that section of his study, he makes a statistical calculation that offers an easy opportunity for someone to check out the marginal value of computers for political science. He seeks to learn the extent to which the changes in the volume of abstentions across time (1876–1965) varied among *départements* and admits that the obvious method for determining this would be to "calculate a coefficient of linear correlation between each consultation." But, he "recoiled before the number of computations that would have required." Instead, he calculated the comparative rankings of the *départements* for the periods 1876–1914, 1919–1936, and 1945–1962. Colleagues who are more friendly with computers than I am tell me that the operation he rejected as too gruelling could be performed quickly and easily by computer. One wonders if M. Lancelot's study would have been more exact if it had been computerized.

Following his general study of the relationship of geography to abstentions, M. Lancelot makes detailed studies of the 1936 and 1946 elections and the 1958 and 1962 referenda. Then, he analyzes the effect of "political" factors on non-voting, both the political context of the consultation and the political attitudes of the abstainers. Next, he examines the relationship of various biological, economic, educational, occupational, residential, and religious characteristics of eligible voters to their tendency to abstain. Finally, he attempts to determine the extent to which the non-voting part of the electorate is stable. Are most non-voters consistently in that category or do they bob in and out, responding to highly individualized stimuli?

As one reads the Lancelot study, one is continuously impressed by the large number of interesting questions about non-voting that deserve serious examination. Who are the non-voters and why? How do they affect the operation of political systems? How do they affect the balance of political power? Furthermore, even a rudimentary knowledge of research resources suggests that non-voting should be at least as easy to study as voting. Most election rolls are open for public examination and indicate clearly who failed to vote. Often, they also provide such information as address, occupation, age, date and location of previous residence, party registration, etc. With very little effort, the serious student of electoral behavior could analyze these data to discover correlations. Also, he could identify non-voters and interview them to learn their motivations, level of information, etc. As non-voters in most elections are much less numerous than voters, his task would be less formidable than if he were studying the voters.

Yet, Lancelot's study is the first general work on the topic. Why have so few scholars been interested in the topic seriously? One hopes that interest hasn't been lacking because of a too-facile willingness on the part of many political scientists to accept the old myths about "apathetic" and "alienated" voters. In fact, field study has shown repeatedly that official sources (at least in the United States) tend to exaggerate the number of non-voters. (An excellent recent example is Kurt Lang and Gladys Engel Lang, *Voting and Non-Voting,* Blaisdell, Waltham, pp. 73–80.)

Indeed, the most striking conclusion to be drawn from the Lancelot study is that non-voting is an extraordinarily complex phenomenon. His research indicates that categories of voters who are well integrated in society tend to abstain less than those who are poorly integrated, but poor integration may be no more than residence in a high Alpine valley from a decent road and farther still from the polling place. He identifies an equally large category of abstainers who are prevented from voting by factors at or beyond the limits of their control, i.e., illness, travel. Finally, he shows clearly that the character of the election affects turnout appreciable. Even these generalizations conceal great variations and contradictions. No category of the electorate that he sets up is wholly exempt from abstentionism and the differences between categories are usually quite marginal.

The student of voting behavior will find much valuable information in the Lancelot study, though he should be very cautious in using it to support generalizations that purport to explain non-voting. Also, his data are very questionable when he moves outside France, especially with respect to the United States. That, however, is not his fault, but the fault of American political science for not doing for the United States what M. Lancelot has done for France.—WILLIAM G. ANDREWS, *State University of New York, Brockport.*

*Political Change in Contemporary France: the Politics of an Industrial Democracy.* BY HARVEY WATERMAN. (Columbus, Ohio: Charles E. Merrill Publishing Co. 1969. Pp. 256. $2.50.)

As a frame of reference for his analysis of French politics, Harvey Waterman draws upon existing theoretic statements offering explanations of the dynamic of industrialized western nations. Having quite adequately summarized the basic conclusions of the literature, he synthesizes and sets forth the following thesis: with industrialization comes affluence, a breakdown in class distinctions, a lessening of traditional cleavages and a decline of ideological dogmatism and conflict. In sum, industrialization and affluence are said to induce inevitably a decline of ideology and a consequent growth of political consensus and stability.

In spite of occasional contraditions and too heavy reliance on impressions, Waterman's rapid review of French political ideologies, style, partisanship, participation and protest affords him the following set of conclusions: There is today an openmindedness in French politics and a willingness of organized groups to cooperate with one another. There has been a decline in ideological rigidity in France with fewer people seeing in political parties, a comprehensive view of society. Dogmatism and exclusiveness within the society have declined and with them political cleavages have dissipated. Fewer people are voting on the basis of the religious categories of the past, but rather on the basis of increased knowledge heightened by an ever growing interest in politics. With this has come an increased tendency to discuss particular issues and to accept compromise in preference to either stalemate or conflict. The people of France are no longer demonstrating an intolerance of governmental institutions, even those of the Fifth republic. There has been a tendency on their part to view government as providing legitimate mechanisms for incremental change and the realization of economic and social goals. Consequently, the political maturity and stability of an industrialized France is asserted.

Waterman runs into difficulties on two levels: first, his argumentation tends to be spotty, uneven and often weak; second, his methodology seems to have caused an over-idealization, and perhaps an oversimplification of the current state of French politics.

A large part of the case built by Waterman is set forth in his discussion of the decline of traditional political ideologies: Marxism; Socialism; Liberalism; "Political Catholicism" (he should have said Catholicisms); "Radicalism and Gaullism" (both in one page); "The Extreme Right: Ultracisme (sic) Algerie Francaise, Nationalism"; Nationalism; "Europe." The problem is that he is not dealing with "Ideology" but with political movements. At the outset, Waterman indicated that the "decline of ideology" must be viewed in terms of a movement along a continuum from one extreme, that of the profusion and adherence of many to comprehensive, articulated world views, toward the other, that of less formalized yet coherent value structures which guide behavior influencing orientations, perceptions and evaluations. But in the main body of the book it is not the doctrine but rather the political movements which are discussed. It is the feeling of the reviewers, for example, that the ideology of the French communists can not be understood simply by recounting the shifts and moves of the Communist party. The fact that Duclos did not advocate communism in the last presidential campaign is no more convincing a demonstration of the decline of ideology than Thorez's *main tendue* to the Catholics twenty years ago. And Guy Mollet's zig-zags (together with so many others) on the issue of laicité neither proves nor disproves anything at all. In the same sense the chapter on Political Partisanship is quite disappointing. Except for the period of the Resistance and that immediately following the liberation, a period when there were but three parties each paying lip service to the slogans and ideas of the "left," an extremely "ideological period," we doubt that the French would have shown any committment to their political parties at any time in their republican history in spite of what Waterman might suggest. Aside from this the author misses the point by giving less than a page to the political clubs and but a bit more than a page to the PSU.

The explanation for what we perceive to be an over-simplification of French politics, is to be found in the author's somewhat uncritical application of the "decline of ideology" theory. Having indicated in his second chapter that most "generalizations about behavior are based on studies made in the United States," he goes on to accept *in toto* their applicability with the assumption that "all such nations [industrialized, western nations] are in many respects very similar" and that the researcher might therefore "use whatever scholarship comes to hand." But Waterman is not alone in his failure to forsee the violent uprisings of May-June 1968. What is interesting is that he felt compelled to write an apologia in the form of an Epilogue. It was not necessary. Nobody expected a movement of violent revolt to occur. But because the traditional ideologies of the 19th century were on the decline there is no reason and there was no reason to believe that the end of Ideology had come.

In the interstices of our industrial society, big, complex and rich (but not so easy), men and women continue to search for the basic value systems that will satisfy them. In so doing, they not only abandon and reject the ideologies of the past (which are indeed on the decline) but seek new values that are expressed in new forms, even if they appear to us to be apolitical, that inevitably shape behavior. This is not a mass phenomenon; it is not a political one; it cannot be easily reflected in surveys. It is initiated by a small group of intellectuals and students (Waterman discovers in his Epilogue a student subculture). Perhaps the time has come for the Clerks to atone for their Treason!—Roy C. Macridis and Theodore Schell, *Brandeis University.*

*Responsible Government in Ontario.* By F. F. Schindeler. (Toronto: University of Toronto Press, 1969. Pp. 295. $8.50.)

Although a case study in the sense that it concerns only the government of a single Canadian province, this book is addressed to the more general problem of how British-model parliamentary institutions can be maintained in modern industrial society. Specifically the author is concerned with the threat posed to the continued viability of parliamentary institutions by a current imbalance of power that favors the executive over the legislature. Schindeler's study of Ontario government during 1945–65 is not an historical one, but of necessity, part of the book is given over to a description of the development of responsible government in the province and to a delineation of other relevant political institutions. The work, it should be noted, also is comparative and theory-relevant and has the additional merit of being lucidly written.

In Schindeler's view, two features are especially important in understanding the Executive's domination of Parliament in Ontario: the short legislative sessions and the tendency toward one-party dominance. Having diagnosed the situation, he suggests a cure, or, rather, a number of "reforms" that are likely to have an ameliorative effect. Not unexpectedly, these include the provision of more research assistance for back benchers and committees, adequate office and secretarial services for members, access to information stored with the bureaucracy, greater opportunity for legislators to participate in the policy -initiation and -evaluation processes, and more effective employment of current procedures such as the question period. He also suggests lengthening the legislative sessions and re-organizing legislative business so that more effective use can be made of the House's time at the beginning of a legislative session.

Schindeler claims that his suggested reforms and innovations would "completely change the style of government that currently holds sway in Ontario." (p. 271). Through them the Government would be made "responsible for its actions, not necessarily to the legislature . . . but to the people who would at least be given some basis for assessing the performance of the Government before casting their votes." (pp. 271–272). Given the low level of interest in politics and the lack of political information that generally characterize Western democracies, and the fact that recent empirical studies suggest that Canada is even a less politicized society than the United States, such a claim may reflect wishful thinking rather than the real world of Canadian politics. In part, the author recognizes this in that he realizes his suggested reforms and innovations constitute a necessary, albeit, not a sufficient condition for achieving truly responsible government. This is, however, a very minor short-coming in a very good and careful analysis of an important problem. Schindeler's study will be especially welcomed by students of federalism and of comparative politics since it is one of the few systematic studies extant to legislative institutions at the sub-national level.—Allan Kornberg, *Duke University.*

*The Affluent Worker: Political Attitudes and Behaviour.* By John Goldthorpe, David Lockward, Frank Bechhofer and Jennifer Platt. (Cambridge, Britain: Cambridge University Press, 1968. Pp. 95. $5.50.)

The background to this study is the fall in the vote for the British Labour party in the 1950s. People interested in the fact that Labour lost three successive elections tried to account for it in a variety of ways. Some suggested that manual workers who had traditionally been voters, were now dropping their loyalty to Labour because of increasing wealth. It was, as we shall see, a vague theory although the problem for the Labour party was real enough.

At the beginning of their study the authors declare "The main objective of this study was to test the widely accepted thesis of working class embourgeoisement: that is, the thesis that as manual workers and their families achieve relatively high incomes and living standards they assume a way of life which is more characteristically middle class and become, in fact, progressively assimilated into working class society" (page 1). The volume under review deals with specifically political aspects of this hypothesis namely the attitude of affluent workers to the Labour party. They conclude "We find that a large and stable majority of our workers are supporters of the Labour party, even though there are signs that in many cases this support is of a rather 'instrumental' kind. This leads us to a discussion of the view that affluence is a factor leading to the decline in Labour loyalties among the working class. . . . Finding little evidence in support of this idea, we then turn to consider two other possible sources of influence on the voting behaviour of the affluent worker . . . a worker's white collar affiliation and trade union membership. Both these variables are associated with our workers propensity to vote Labour." (p. 1). So much for the statement of aims and their summary of their findings.

An immense amount of work has gone into this study of which we are promised four volumes. The question is whether they were studying a real problem and whether they went about it in the correct way. I believe that the answer to both these questions is, no. I shall take them in turn.

The authors study a group of 'affluent workers' in Luton in order to test whether embourgeoisement has taken place. Although the general idea of this process may be fairly clear any research worker must define it somewhat closely before he can claim to have confirmed or disconfirmed it. My first complaint is that this is precisely what the authors have failed to do. They admit to this. "Our study does not allow us to make as thorough an investigation of affluence as would be desirable to test the affluence thesis decisively. We have noted that this thesis itself in some respects imprecise and would need clarification and refinement in order to be tested in any definitive way. However, since we ourselves do not embrace it we shall leave the task to those who do." (p. 46). But four volumes and a half a dozen papers seems an excessive mileage to get out of something which does not even claim to be definitive test. How can the authors claim to have tested a proposition in any sense when by their own admission they do not know what they are testing.

The one hard finding which they seem to have is that Labour support among the affluent working class in Luton is extremely high. Somehow this seems to be a disconfirmation of the "affluence hypothesis." So it may be but only when it is related in some way with a measure of Labour support among manual workers who are not affluent. We are not offered this type of comparison. Instead within the group in Luton those earning more than £18 a week are compared with those who are making less. This is really not very satisfactory since we are told at the beginning of the volume that they are all "affluent." In any case the effect of social context and community might very well be such as to make even the more poorly paid different from groups of equivalent income in other less affluent locations.

The authors do not, however, stop at this in their study of the possible effects of greater affluence. They ask a series of questions, which are supposed to divide middle and working class respondents. In three of the four questions the response of the majority of manual workers is the same as the response of the majority of a white collar control group. In the fourth question the manual workers are very evenly balanced. The authors admit that this constitutes some evidence for the embourgeoisement hypothesis but once again the only way in which this data could have been made really meaningful would have been in comparison with a sample of workers who were not affluent. Again the authors find that their affluent workers have what they call an '"instrumental" attitude towards the Labour party. By this they mean that when asked why they support Labour a large percentage answers in terms of the Labour party giving a better deal for the working man. Is this really a test of an instrumental approach? The fact that a worker verbalises his reason for supporting the Labour party in terms of its use to his class really has to be supported by other sorts of evidence. "The party of the working class" is at least as much a symbolic statement about Labour as an expression of self interest.

In the long run they conclude, as we have seen, that affluence is not important in determining loyalty to the Labour party. White collar affiliation and trade union membership are much better predictors of Labour or non Labour voting. This is shown quite clearly but on the other hand this does not prove that affluence is unimportant. It is fairly widely accepted that white collar affiliation is an important cross pressure for the working class. Indeed the authors could profitably have gone into it in more detail. Its full theoretical implications, which are extremely important, are not spelled out. Their data about the relationship of white collar affiliation and trade union membership is used to prove that when they are taken into account the relationship with affluence disappears. But in pages 57 to 59 where they try to prove that the effect of affluence disappears when other factors are taken into account, they control on the wrong variable.

I now turn to some final statistical criticisms of the study. In the first place the authors use what they call a critical sample. This device is thought to form the basis of a test such that "if, in the case studied, a process of embourgeoisement was shown not to be in evidence, then it can be regarded as extremely unlikely that such a process was occurring to any significant extent in British society as a whole." (Page 2). They argue that Luton constituted just such a case. Here for various reasons which they outline was the "affluent" town. But how can we say that Luton is the perfect test case for the embourgeoisement hypothesis when it is by now clear that no one has quite defined what this hypothesis is. We are given some rather vague information about Luton but nothing which compares it with other areas. Above all there is simply not enough argument in favour of this unusual research strategy of "the critical sample" to make it sound even vaguely convincing. How are we to know that some distorting factors may not be at work in Luton. At least with a more widely selected sample we might have been able to allow for "community effect." But worse is to come.

The authors agree (page 84) that their is not a random sample of the electorate. It is therefore not possible to generalise even to the population of Luton. It deals only with certain occupational groups within three firms and among these it only deals with men (not women) who are married and who are between 21 and 46. As a sample of any kind of the working class population this is beginning to look extremely odd. Of this sample, however, no less than 19.6% refused to be interviewed (page 5). What effect this might have upon the analysis can only be guessed. The authors make it even more difficult to guess by giving us no idea of who these refusals were. Indeed the whole methodological description, apart from the somewhat tendentious discussion of "the critical sample" is so brief as to be almost useless. They do not even publish their questionnaire. One is led to suggest that their data is in rather a bad state or at any rate there is little evidence to give us confidence in it.

One last comment is in order. Throughout the entire book not one statistical test is used. Given the fact that they were not using a probability sample it is understandable that they find simple measures of association inappropriate. On the other hand multivariate statistics would certainly have been in order to look at the relation between the various dimensions. The fact that they have not been used is a sad comment on the extent to which British sociologists are able to cope with the tools which are readily available.—JOHN BRAND, *University of Strathclyde.*

*Guatemala, la Violencia.* COMPILED BY ALEJANDRO DEL CORRO. (Cuernavaca, Mexico: Centro Intercultural de Documentacion, CIDOC, 1968. Vol. 1, PP. 114; Vol. 11, Pp. 135; Vol. III, Po. 435. Obtainable by subscription to the CIDOC Dossiers.)

What makes a political order breakdown into widespread violence and civil war? To answer this classic question, political scientists are currently turning to comparative quantitative analysis, but the detailed studies of individual cases are also needed. One of the most interesting, but tragic, recent cases has occurred in Guatemala where internal dissension and violence ascended to a peak in 1967 and 1968 of a virtual war of all against all, without, however, having the attention-gathering result of an overt U.S. intervention or a communist takeover. Neither U.S. scholars nor press have fully reported the situation, let alone analyzed it, but the Intercultural Center of Documentation in Cuernavaca, Mexico, has advanced the study immensely with the publication of three volumes of bibliography and documents. To my knowledge, no similar collection has been published on any other case of civil war, revolution, or widespread violence.

The shorter first two volumes contain bibliography and reprints from Guatemalan newspapers, principally *Prensa Libre, Impacto, El Grafico,* and *La Hora.* The third and longest volume contains a bibliography and the reproduction of documents "of difficult acquisition." Included are statements by Guatemalan groups of every ideological orientation. Many of the pieces were originally only mimeographed and circulated clandestinely. Others were circulated publically as handbills. One item is a thorough and detailed history of the rebellion to 1965 by an unnamed foreign source.

None of the documents presents a sufficiently complete description of the events to allow an assessment of the severity of the Guatemalan violence in comparison with other contemporary breakdowns in public order. However, enough material is given to indicate that insecurity was complete in all the non Indian communities of the country. In these areas, no one of any class or occupation, official or unofficial, was safe from assassination and mutilation, seizure and torture, or balckmail. Strangely, the most oppressed group, the 60 per cent of the population composed of unassimilated Indians, refused to become involved. No lines between organized fighting forces were drawn. The rebels machine gunned police or shot up "traitors" in the center of Guatemala City. In retaliation the "anti-communist" unofficial groups or the secret police dragged persons from their homes, tortured them, and left their bodies in the countryside. At the same time guerrilla bands roamed the country and were pursued by the army who struck against anyone who might have collaborated.

Many different perspectives of the violence are found in the documents. The rebels, the moderate political parties, the governments, the army, and the rightwing groups have different descriptions of the facts, and varying interpretations of the causes of the violence. The total impression of the different pictures together is baffling, but it may give a better representation of the reality than would any single "objective" interpretation. The collection certainly precludes any simplistic or one-sided interpretation of the events.

To add to the confusion, groups continued to change their views over the years in which the violence occurred. The communist party, *Partido Guatemalteco del Trabajo,* for instance, began by opposing the rebellion and accusing the participants of being ultra-radicals and irresponsible "provocateurs," but it ended by giving its wholehearted support to the guerrillas and encouraging their partisans to renewed efforts after the tide had turned against the rebellion. The original rebel forces split, and one of the original leaders, Luis Turcios, accused his former comrades of Trotskyism. The Catholic Church went from a rigorous anticommunist position to opposition to all violence but in favor of social change. The intense anti-communist propaganda and terroristic campaign of the right wing groups, MANO, NOA, MNL, CEDEG, came into being only after the elected government of the *Partido Revolucionario* assumed office in 1966 and sought some sort of accommodation.

These volumes could be well used as readings for students with ability in Spanish instead of the bland statements so often used. They provide enough material for numerous political analyses at all levels of scholarship. Most important, they contribute to a more profound understanding of one of the most critical issues of all times, the breakdown of public order. The contents deserve to be far more widely disseminated than is possible under the subscription system of CIDOC dossiers.—JOHN F. MCCAMANT, *University of Denver.*

*Technocracy.* BY JEAN MEYNAUD. Translated by Paul Barnes. (New York: The Free Press, 1969. Pp. 315. $12.95.)

Ever since the dawn of industrial society, students of politics have been concerned with the question of the relationship of technical expertise and democracy. But although the problem of the expert is as ancient as Plato, it is only in modern times that the technical specialist's claims to superior insight have been generally accepted. In recent decades the difficulty of reconciling the knowledge of the minority and the power of the majority has become so intensified by the growth of science and technology as to give birth to what is virtually a new field within political science, the study of the interrelations of science and politics.

France has long been a polity in which a centralized state has made maximum use of technical expertise, and there has been a widespread tendency to refer to the Gaullist regime as "technocratic." Therefore a book on technocracy by one of France's most distinguished political scientists inevitably holds out the promise of shedding new light on a central problem of modern politics—the impact of science upon democratic government in advanced industrial societies.

For these reasons Professor Meynaud's book is a special disappointment. Part of the fault lies in its basic conception. Its title conceals the fact that in French usage *technique* is a term of such wide connotation as to be virtually useless for analytical purposes, as Jacques Ellul's *The Technological Society* exemplified some years ago. For the French generally, all expertise is "technological" and, following this logic, Meynaud is as much if not more concerned with the role of military and financial experts as he is with scientists in the American sense. He therefore has been forced to tackle a subject so vast as to be almost unmanageable. Focusing on a small part of it, he concentrates on the political role of the technically trained bureaucrats—typically the products of the *Grandes Ecoles.* As a result, his book will be of interest primarily to the student of comparative bureaucracy rather than to one concerned with the larger interrelationships of science, technology and politics. *Technocracy* is also (and here the author is perhaps reflecting a general weakness of French political science) an extremely parochial book and assumes much detailed background in French government and administration, including minor ideological and political conflicts. Reference to British and American experience is marginal, confusing and occasionally fantastic. For example, re the American presidency, Meynaud comments: "We need simply to notice that over the last few decades (especially after the reforms of 1939, following from the report of a three-man commission, which specially recommended the piecemeal development of a 'fourth branch' with no direct responsibility to anyone) the President's immediate staff has continually increased." (P. 277) This is unfortunately only an extreme example of a confused text rendered doubly unfathomable by inadequate documentation.

Though Meynaud has many ideas and insights to contribute to the general discussion of bureaucracy, they are obscured by generally bad writing made more impenetrable by a bad translation. His style is extremely discursive, though rarely graceful, and it is often difficult to tell whether he is expressing his own ideas or reporting those of others; thus his position remains obscure. In addition, technocracy is dated. The original French edition was copyrighted in 1964, though apparently some footnotes were hastily added to the British edition of 1965 which is what the Free Press is reprinting here. Nor are the added footnotes of much use; they are frequently incomplete and in some instances completely incorrect; e.g., p. 102 where a textual reference to a book by Fred J. Cook is footnoted by the incomplete citation of a work by

(Maxwell) Taylor.

Mores the pity. For Professor Meynaud is an eminently humane and sane observer of the phenomena he discusses. He recognizes that the role of the expert cannot be treated in abstraction from general political and economic systems and that at present in France—and elsewhere one might add—bowing to the expert means not only or even perhaps primarily yielding to informed reason but also means the enhancement of the power of the existing capitalist elite, pace James Burnham and his followers, whom Meynaud refutes at length. He also realizes that science is not a menace as such and that, although technology opens up new possibilities of the diminution of human freedom, they will only become actualized if the electorate and political leadership of democracies permit them to be. Science presents potentialities, but politics is paramount in determining the future. Deeply concerned that men learn to master the new technical elite which threatens to make formal democracy meaningless, he attempts briefly to prescribe how this might be done. But his suggested methods of strengthening control by the people—"Redistribution of authority within the Executive"; "Reevaluation of parliamentary influence"; and "Modernization of political forces"—manifest the same weaknesses as the book as a whole in being simultaneously too general, too specific, and too narrowly French in context.

Thus Meynaud's *Technocracy* is a more sound but less interesting book than his countryman Ellul's influential polemic. But it is simply not in the same class with the work of Robert Gilpin on France or with that of men such as Don Price and Donald S. Greenberg on the general problems of the relationship of science and politics. A revised and tightened edition of *Technocracy* might have been of some use to American social scientists, but the present publication is a disservice to the reputation of the author and the publisher alike.—VICTOR C. FERKISS, *Georgetown University*.

*The Soviet Police System*. EDITED BY ROBERT CONQUEST. (New York: Frederick A. Praeger, 1968. Pp. 103. $5.00.)

*Justice and the Legal System in the USSR*. EDITED BY ROBERT CONQUEST. (New York: Frederick A. Praeger, 1968. Pp. 152. $5.00.)

The two studies appear without assigning their preparation to any specific person or persons. In the preface credit is given by the editor to Messrs. Murray, Friedman and A. Alekseyenko for collaboration in preparing the volume on police, and to Messrs. Alekseyenko and Murray for cooperation in the study on *Justice and the Legal System in the USSR*.

These two slim volumes are useful as systematic organizations of material and information on the role of police and of the law and courts in Soviet society. They cover the life of law and of the police from the beginning of the Soviet regime in Russia including the latest phase following the post-Stalin reforms. Indeed these reforms provide a focus for the inquiry which the editor and his collaborators have outlined: What is the meaning of the reform which followed the demise of Stalin? What is the practical significance of the rejection of Stalinism, and of the quest for the socialist legality?

The result of the review of the history and of the present condition of the Soviet polity together with its legal and political institutions, leads the editor to conclude that in its fundamentals the regime changed but little. The Soviet state was still a Stalinist state, and whenever it felt that the interests of its policy required it, had not respected its own laws. And ". . . so long as the Soviet state maintains its rights to override not merely in extreme emergencies, but in all cases where its interests are involved, the letter and spirit of its own laws, advances towards general legality, have no guarantee of permanence."

The same applies to the workings of the police apparatus, which still continues to operate in a basically Stalinist manner:

"For there has been no abandonment of the police machine's role as an institution for controlling population in the interest of leadership . . . regardless of law and public opinion."

The editor and his collaborators have relied primarily on the original material and Soviet writings, and have disregarded—with few exceptions—Western literature on the subject. While this is a practice followed much too frequently by other scholars in the field of legal studies, in the case of the two studies edited by Mr. Conquest, this practice is grievously wrong, and detracts seriously from the value of the two books. The impression these two volumes will create is that they are the first efforts in the field, which is quite untrue. Those who would start the study of Soviet institutions by using the studies under review were given no guidelines where to seek additional information, in the works of authors who have been working with significant results in the field.

Not only are individual authors active in the study of Soviet law and institutions not mentioned, but there is no information regarding the periodical literature, translations of laws of the books and Soviet studies into the Western European languages published in this country, Germany, Holland, France, England, Austria and Belgium.

The omission is even more serious, owing to the slimness of the volumes under review and the span of time of the development of Soviet legal institutions involved. Frequently coverage of key issues is quite inadequate, and some of the institutions are altogether omitted. It is not suggested that the format of the books as such rules out their usefulness for the study of Soviet legal institutions, but that absence of reference to additional literature available in English and other Western European languages has highly limited their usefulness.—KAZIMIERZ GRZBOWSKI, *Duke University*.

# INTERNATIONAL POLITICS, LAW, AND ORGANIZATION

*The Politics of Peace-Keeping*. BY ALAN JAMES. (New York: Frederick A. Praeger for the Institute of Strategic Studies, 1969. Pp. 452 $11.00.)

For very nearly a quarter of a century, the United Nations has expended words—by the billions—and deeds—far more than memory easily retains—to maintain both local and general peace. Merely to outline the ceaseless activity of the world organization represents a huge effort of bringing together the records of meetings and field expeditions and the more fugitive evidence of negotiations, deals and pressures. To produce from this vast mass of material a comprehensive account in a well-organized framework is a real accomplishment. Alan James, Senior Lecturer at the London School of Economics and sometime Rockefeller Research Fellow in International Organization at Columbia University, has done precisely this in *The Politics of Peace-Keeping*.

This tightly-written volume includes every effort under United Nations auspices to deal with active conflicts that get attention at the international level, except for those involving collective security in a closely-defined sense. James concerns himself with the goals of individual states as they interact and with their maneuvers to protect or advance their interests as they define them. From these maneuvers emerge the conflicts that come into the purview of the United Nations. The organization produces influences of its own, which are directed towards, and sometimes achieve, changes in state behavior. James does not seek and expose the causes of state behavior within the domestic polity. Nor does he project state behavior on the broad background of a theoretical system. His explanations and rhetoric hew closely to those of the diplomatic historian, although his analytical framework does not tie his study to a time scheme. His explorations of diplomatic alternatives available to governments involved in conflicts, either directly or through the United Nations, are sensitive and ingenious.

James employs a wide definition of peace-keeping, so broad as to revise drastically the conventional conception of this form of United Nations work. It encompasses all of the usual modes of peaceful settlement of disputes, such as negotiation, mediation, enquiry, etc., as well as the more recent organizational innovations in the form of non-violent employment of military forces which have been dubbed "peace-keeping" since Dag Hammarsjköld and his associates discovered Chapter Six-and-one-half of the United Nations Charter. Any national effort to which the United Nations responds as a proximate or distant threat to the peace falls within the definition. Aggression and response to it by enforcement action involving Security Council decisions of binding legal effect and invoking the combined might of the great powers if excluded. Not much in what the United Nations calls the political field, as well as in the treatment of colonial problems, falls outside of this definition. James concentrates on overt, perceived disputes and conflicts; this excludes by implication those methods of promoting peace by such means as general arms control systems or raising the level of general welfare which, some assume, reduce international conflict.

In order to deal with incidents that range in size and notoriety from the Buraimi Oasis dispute to the Congo, James devises an intelligent and reasonably precise set of three main categories and 10 sub-categories. He assigns incidents to the categories on the basis of goals expressed in United Nations resolutions, modulated by the contents of debates and governmental statements and his own estimates of intentions. Inevitably an affair as complicated as, for example, the long-enduring Palestine case changes in the course of treatment by the United Nations and must therefore be split among the sections of the book. Useful cross-references restore some unity to the resulting fragments.

In James' scheme, the first category, "Patching Up," includes efforts in the United Nations to bring about the settlement of disputes or at least reduction of conflict to a tolerable level. Cases are analyzed within four sub-categories of activity—impartial investigation; mediation; supervision, which covers United Nations assistance in carrying out an agreement between disputants; and administration, which includes short-term or longer international rule.

The second and third categories, "Prophylaxis" and "Proselytism," are more novel in conception and content than the first. The sub-categories of prophylaxis, intended to keep a dispute from getting any worse without settling it include accusation, or the use of reporting to discourage objectionable behavior; sedation, or calming efforts among which are negotiations and "cooling activity at the military level," (p. 260) as in the Palestine Truce Supervision Organization; obstruction, or the placing of a force, such as the United Nations Emergency Force, between the contenders; and refrigeration, or the exercise by the United Nations of governmental functions in a contentious zone without agreement on the eventual outcome, as was proposed for the former Italian colonies.

"Proselytism" encompasses efforts to ". . . upset certain aspects of the established order. . . ." (p. 9) by means of invalidation, or the use of reporting and investigation to show "that a regime has lost all moral right to continue its rule," (p. 372), a procedure employed continuously by the Committee of 24 on colonialism. Proselytism also includes coercion (but not of the collective security type) as used in Rhodesia and in the Congolese province of Katanga.

On the basis of this subtle differentiation of various forms of peace-keeping, James attaches to the consideration of each a cautious estimate of the likelihood of its future use, always calling attention to the national policies and power alignments required as preconditions. "Certainly," he generalizes, "UN peacekeeping seems to be firmly established as one of the processes to which states can resort for the furthering of their international purposes." (p. 439) Again and again, he emphasizes his belief that the use and success of peace-keeping efforts depends ultimately on the attitudes of the governments concerned and, for the naive, remarks that United Nations peace-keeping neither ends politics nor raises its moral tone. Rather it offers a means for obtaining stability, a goal which he asserts underlies the bulk of peace-keeping.

It is a source of cohesiveness and consistency in this study that its author clings so tenaciously to his vantage point at the level of state policy. Yet this prevents him from commenting on the development and future of the United Nations as an international institution that is something more than its individual members and something less than a government. Implicitly his work recognizes the United Nations as an apparatus of this character, as he writes, for instance, about attempts by the organization to send a group of observers, or as having a defined point of view, but he nowhere makes explicit his estimate of the influence the institution has on international politics generally.

In as meticulous a book as this one, it is regrettable to find the Dominican Republic always called "Dominica," a minor Caribbean island with a British colonial background. Insertion of a chart of cases, using James' analytical scheme, would have improved this already very useful study.—Leon Gordenker, *Princeton University*.

*Neutralization and World Politics*. By Cyril E. Black, Richard A. Falk, Klaus Knorr and Oran R. Young. (Princeton: Princeton University Press, 1968. Pp. 165. $7.50.)

In the spring of 1966, four members of the Center of International Studies at Princeton University undertook, at the invitation of the Senate Foreign Relations Committee, a study of the applicability of neutralization to the problems of conflict in Southeast Asia, especially Vietnam. Out of this experience grew the present, more general, study. Meant as a contribution to contemporary International Relations literature, it is a clear, well-organized though preliminary exploration of the principal issues involved in the concept of neutralization, its risks and its advantages. The authors have produced a coherent argument and a craftsmanlike review of historical precedents and practical problems. Individual contributions are not identified and have been well integrated. But this unity of result has also been achieved at the price of a certain blandness and lack of individuality reminiscent of committee-negotiated government documents.

In the tradition of international law, the writers see neutralization as "a formal status of permanent neutrality" suitable or potentially attractive "only for relatively minor states." They define it more technically as "a special international status designed to restrict the intrusion of specified state actions in a specified area" and distinguish it from demilitarization and neutralism. The state actions referred to are mostly the activities of Great Powers: non-interference in domestic affairs and various types of military activity with respect to a particular territory. They see the establishment of a neutralized status to rest upon prior conditions of balance, both of power and of the objectives of the major states involved: a military stalemate and an equilibrium of resolve. A negotiated neutralization agreement translates the condition of military stalemate into that of political stalemate.

While the initial agreement to neutralize thus rests upon a stalemate between the Great Powers, the viability and the maintenance of neutralization is posited upon the strength of national institutions in the particular area. Local stability also removes from the neutralized state the odium of inferior or protected status. The vitality of Swiss neutrality rests upon indigenous resources of organizational capacity; but these have been lacking in Laos and have jeopardized the settlement negotiated there in 1962, and they are those found most wanting in Southeast Asia in general. Yet, as the authors recall, neutralization need not necessarily offer solutions for eternity. If it is a formula that succeeds in organizing and maintaining the settlement of a conflict for a period of twenty to fifty years, it may be said to have achieved its purpose.

With regard to Vietnam, the book presents some caveats and few solutions; "'there is, of course, no point in drawing an overly optimistic picture of the prospects of achieving a neutralization agreement for South Vietnam." The principal consequence of such an agreement would be to provide a "framework within which a process of political shakedown" including "violence of substantial proportions" "could take place without endangering international peace and security." "The outcome would constitute the closest approximation to self-determination that South Vietnam is likely to achieve in the near future." While the Paris talks, begun since the completion of this volume, crucially revolve around the question of troop withdrawals, the concept of neutralization as such does not appear to have become a negotiating objective of either side.

The dominant note of this study is caution, even modesty, in advancing the merits of neutralization. "Neutralization is a concept of limited applicability" reads the opening page of the preface and the feeling prevails that the authors are concious of addressing a sceptical, if not critical, audience; they seem to be trying hard to be reasonable, pragmatic and sensitive to the 'realities of world politics.' This is particularly apparent in their discussion of the applicability of neutralization to larger states or to more important areas such as Europe. With all this caution they may have taken the attractiveness out of a promising idea which should be of theoretical interest to all students of International Relations. For if neutralization is indeed a method for resolving conflict between the Great Powers, arrived at by way of stalemate, it could have wider applicability: potentially, to all areas of the global system.

The 'world politics' which the authors accept as the unquestioned framework of their analysis is only one particular type among possible world orders. It is a type of world politics characterized by a high incidence of major wars, and the preeminence of Great Powers deriving from their role in wars and their settlement. True neutralization is incompatible with this system because the conditions of war and Great Power control undermine the viability and diversity of domestic institutions in the lesser states, on which lasting neutralization must rest. Conversely, a situation of global stalemate of power and resolve might conceivably create conditions in which the neutralization of large areas based upon the absence of war and upon indigeneous strength would prove a plausible alternative to the contemporary system of Great Power politics.—George Modelski, *University of Washington.*

*Supranationalism and International Adjudication.* By Forest L. Grieves. (Urbana: University of Illinois Press, 1969. Pp. 266. $10.00.)

Those interested in evolving international institutions and in burgeoning "world law" will find in Forest Grieves' comparisons of five international courts (the Central American Court of Justice, the Permanent Court of International Justice, the International Court of Justice [ICJ], the Court of the European Communities [CEC], and the European Court of Human Rights) a useful handbook and a compact analysis. The author devotes one chapter to each court, introduces them all with a chapter on "Sovereignty and Supranationalism," and follows them with his "Summary and Conclusions," eight appendices (57 pages of selected articles from the constituent instruments and a table of CEC cases), and sixteen pages of bibliography.

Grieves' introduction points out that *supranationalism* is a new political term, having been used privately only as far back as the 1940's and in international public law for the first time as recently as 1951 in the treaty establishing the European Coal and Steel Community. By reminding us of these facts, Grieves demonstrates both how contemporary and how important his subject is. After discussing the ambiguity that inevitably surrounds so new a word, he joins Werner Feld in defining the term to mean "that signatory states have transferred to an international institution certain limited decisionmaking powers normally exercised only by the governmental organs of a sovereign state, powers which include the capability of issuing, under certain specified conditions, binding norms to the states or to their inhabitants" (p. 14). By this criterion, only the CEC qualifies as supranational, and Grieves makes his point sharply by singling out the case of *M. Flaminio Costa v. E.N.E.L.* (1964), where the CEC successfully upheld a Community norm in the face of a decision by the Italian Constitutional Court that ignored the Treaty of Rome. Grieves discusses individual cases only when they bear directly on supranationalism, but his lists and tabulations of contentious cases that have come before each of the courts makes his book a helpful compendium of the courts' jurisprudence.

Grieves measures the courts' supranationalism by examining their constitutional bases and their practice, which seems reasonable enough. But he also looks at the courts in terms of the expectations of statesmen instrumental in bringing them into being, hoping (p. 16) that the "supranational views of leading statesmen during the *travaux preparatoires* . . . are relatively indicative of the milieu within which an international tribunal was founded." In this view, he cannot be challenged, but it is puzzling that, while admitting that expectations are "not always a particularly reliable indicator of a court's power," he persists in classifying expectations as a measure of power. Because statemen's hopes are interesting and important, they could certainly stand on their own as a contrast to later realities. Treating them just as background material would have been far more legitimate and would, moreover, have made Grieves' presentations less mechanical than they now seem. The rigid parallelism of his five central chapters facilitates comparisons, no doubt, but it is a rather primitive mold into which to press his study. A profusion of subheads and numbered or lettered sections heighten one's feeling that one is reading an outline rather than a completed work. The pedestrian handling of the much-vexed problem of sovereignty with which the book opens is no adornment either and emphasizes the book's more routine aspects. One is also put off by Grieves' leaning somewhat heavily on John Herz's "Rise and Demise of the Territorial State" without taking account of his later "The Territorial State Revisited."

If, however, Grieves starts out by over-stressing the state's decline, his strict focus on supranationalism and adjudication keeps him in one instance from doing full justice to the courts' impact on the nation-state system. He fails to point out, in discussing access to the ICJ (pp. 91–92), that UN organs and specialized agencies authorized by the General Assembly may request advisory opinions of the ICJ. This omission is all the more unfortunate because he has earlier (p. 9) referred to the Advisory Opinion on Reparations for Injuries Suffered in the Service of the United Nations to show that "international organizations . . . have assumed a juridical personality." Later (p. 137), too, he gives full marks to the CEC for permitting entities other than states to appear before it and cites its capacity to "render advisory opinions in connection with the treaties" defining the European Community. Advisory Opinions are not, of course, *adjudication*, and *adjudication* is this book's subject. Technically speaking, therefore, Dr. Grieves cannot be faulted. But there is an inconsistency here, and access to the ICJ for advisory opinions does cast light on supranationalism.

Grieves has worked with original materials and, where necessary, translated them, though not always felicitously. At times his attention to our linguistic needs seems a bit fussy, too, as when he writes (p. 14) of the constituents of Pierre Pescatore's definition of *supranationality:* "common interests *(intérêts communs)* . . . real powers *(pouvoirs réels)* . . . and autonomy of this power *(autonomie de ce pouvoir)*." He is also carried away one page earlier, where he burdens us with "Fig. 1 Spectrum of Interstate Bonds *(Staatenverbindungen)*," as if the German word added anything.

Despite these gratuitous displays of erudition, Professor Grieves does us a service by emphasizing the need for political and social consensus if international adjudication is to succeed and in demonstrating by an extreme negative example (the Central American Court) and an extreme positive example (CEC) how important spirit and practice are in contrast to expectations and constitutional documents. His comments on judicial impartiality and on our inability to measure it adequately are provocative, and his endorsements of the CEC practice in not using *ad hoc* judges and in keeping its votes secret are both modest and apposite. He would like to see the ICJ adopt the same procedures, but he realizes full well the difficulties standing in the way of such changes.

Grieves quotes (p. 174), endorses, and confirms Hersch Lauterpacht's view that "there are few rules of modern international law . . . more widely acknowledged than the rule that the jurisdiction of international tribunals is derived from the will of the parties," and he shows us again that, by and large, states are still unwilling to preside over their own demise.—RICHARD N. SWIFT, *New York University.*

*Conflict and Consensus in Labour's Foreign Policy 1914–1965.* BY MICHAEL R. GORDON. (Stanford: Stanford University Press, 1969. Pp. 333. $8.95.)

Political scientists from a number of specializations will find Gordon's book rewarding. It will be welcomed by those who are concerned with the discovery of identifiable constants of political behavior over a long period of time; it will provide a useful case study for those who seek evidence of the relationship between ideology and attitudes on foreign policy; scholars specializing in political parties will find a gold mine of evidence concerning factional strife as a component of the democratically-structured party; those oriented toward the study of political leadership will be interested by the author's clear description of the interaction between the Labour Party elite and its challengers within the Party. All will admire, I think, Gordon's capacity for reducing a substantial body of intractable material to a systematic formulation of principles and problems.

On this latter point Gordon is quite clear. He is concerned with accounting for the nature of the conflicts that have divided the Labour Party in the area of foreign policy and with identifying the causes of their persistence. He stays always within the strict limits that he has set for himself, using the history of the Party's foreign affairs controversies since 1914 primarily for analytical purposes.

The first problem approached is the analysis of socialist foreign policy as perceived by Labour partisans. Relying on a four-fold classification derived from Richard Rose, the author proceeds to dissect Labour's loyalty to the principles of internationalism, international working-class solidarity, anti-capitalism, and anti-militarism (conceived as antipathy to power politics and accompanied by varying degrees of pacifism). The description of these four tenets of socialist foreign policy could hardly be improved upon for accuracy or comprehensiveness. This exposition is important because it reveals unequivocably the distance separating the ideological slogans themselves from the content which the slogans have concealed. Indeed, it has been one of the most disconcerting characteristics of the Labour Party that it can claim, after the event, to have formulated a clear-cut policy when an investigation of its actual position at the time will often reveal a good deal of ambiguity. The Party's real talent in the area of foreign affairs often lies in its ability to manipulate principles that have clearly perceived symbolic appeal.

The book is divided into two parts, the years between the two world wars and the post-World War II period, with a brief connecting chapter on "Socialist Foriegn Policy and World War II." The section dealing with the years 1920–1939 brings out clearly the Party's difficulties over a League of Nations-oriented policy of collective security. These years were crucial, as the author points out, for they provided the laboratory in which Labour could have tested its fundamental assumptions about the nature of interstate relations and might have adjusted in a responsible way to the world of reality. Eventually adjustments were made, but only at the approach of World War II and with the agonizing and protesting Party being dragged along by the inexorable march of events. Collective security meant, until two years before the war, disarmament, arbitration, and the mobilization of an anti-war public opinion. Indeed, Labour's attachment to Socialist principles leads Gordon to conclude that "to most Labour partisans disarmament and collective security were at bottom one and the same thing." Even after Ernest Bevin forced the Party to revise its stand on armaments, there was still the puzzling opposition to the introduction of conscription in the spring of 1939. Even though war did come, there was no serious attempt in the Party to re-evaluate socialist principles in the light of the international event of the interwar years. "Right up to the end of hostilities," writes Gordon, "the thrust of party utterances fostered the widely diffused belief that socialist foreign policy was as intellectually sound and morally compelling as ever. . . ."

After 1945, however, a startling change occurred. When the Soviet Union unleased her cold war policy against Great Britain, the Party leadership responded in kind. With Bevin at the Foreign Office, the Labour leadership maneuvered the application of a traditional foreign policy based on the availability of armed forces as a deterrent, the need for alliances, and the admission that the most far-reaching national rivalries did in fact exist. Attempts were sometimes made by the leadership to identify the newly-accepted realism with the long-standing socialist principles, but the depth of the change was nonetheless unmistakable. The sniping of the socialist purists (or "fundamentalists") gradually subsided and by 1950 a near-consensus had been maneuvered by a Party leadership skilled in the techniques of educating the rank and file in the principles of realism.

The Korean War, however, shattered, irretrievably as it turned out, the process of adaptation. The re-emergence of a strong current of socialist orthodoxy, eventually marshaled by Aneurin Bevan against the Party leaders, was the signal for the eruption of anti-Americanism, neutralist currents, renewed pacifism—in short, the re-appearance of bitter doctrinal disputes which still plague the Party. One of Gordon's more startling findings is that for the past several years the two forces, orthodox and revisionist, have been almost evenly balanced. That, in itself, would account for the policy of drift of the Wilson Government. In spite of the well-known powers of the British Cabinet, it is difficult to apply any consistent foreign policy when the party in power is so divided by principles that neither faction can impose its will on the other.

Still, there remains the marked disparity in treatment given to the periods before and after 1945. More than half a century is the time span indicated by the title, and yet nearly three-fourths of the book is devoted to the last twenty five years of the period covered. That this approach can be justified in terms of relevance to today's international scene is doubtful. For while the years before 1922 are dealt with in the briefest fashion, the post-1951 period is given an almost equally sketchy treatment. In reality, the heart of the work is the admittedly important era of adaptation to power realities, 1945–1951.

In his truncated account of the period 1920–1939, Gordon says little about Labour's hostility toward the peace treaties, particularly to their territorial provisions. In reality, the Party's rejection of any military props to collective security in Europe was not due solely to its socialist ideology; it was indissolubly linked to the fear that arms might be used to reinforce territorial boundaries that were thought to be intrinsically unjust. Labour propaganda on that score was unmistakably similar to the claims of German nationalists and militarists and yet neither MacDonald nor Henderson, aware as they were of the real situation inside Germany, made any meaningful effort to reorient the Party away from its pro-German bias. Nor does the author take into account the persistently anti-French posture of Labour. The ultimate importance of this attitude can be debated, and yet neither the impact of Anglo-French differences on the crumbling of the postwar order nor the Labour Party's contribution to the debacle can be ignored.

The author's omissions or near-omissions along these lines probably stem from his arbitrary division between the "progress" years of the 1920's and the "crisis" years of the 1930's. Gordon believes that Labour's "socialist policy fared well enough in the earlier but not in the later decade." But did it fare well in the earlier decade? To say that the crisis of the thirties was due to "'circumstances" or to "a fateful turn" of events is to deny the essential unity of German nationalist goals during both decades. Gordon attributes too much perhaps to the influence of socialist ideology and too little to specific aspects of Labour attitudes that were sometimes more "liberal" than socialist.

In spite of these reservations, we can be grateful for a work that applies admirable scholarship to the exploration of an intriguing subject.—William R. Tucker, *Lamar State College of Technology.*

*The UN and the Middle East Crisis, 1967.* By Arthur Lall. (New York: Columbia University Press, 1968. Pp. 322. $10.00.)

This work is essentially an account of negotiations within the United Nations and a catalog of UN actions relevant to the Arab-Israeli conflict during a six-month period in 1967. The author prepared his study while serving as Adjunct Professor of International Affairs at Columbia University. His sources are official UN records and information supplied by the principal actors at UN headquarters. His access to the latter was facilitated by personal contacts dating from the time when he was Indian Ambassador to the UN. Although his work abounds with citations from official documents (its *Appendix* alone contains the text of twenty-five resolutions that were proposed during 1967, ten of which were adopted by the Security Council or the General Assembly), there are virtually no footnotes indicating sources for information gleaned from his extensive interviews and conferences.

The author explores no hypotheses, and he advances no general theories. Nevertheless, it is clear that he regards peace in the Middle East, together with safeguarding the territorial integrity of all states in the region, as desirable goals for the international community. Equally clear is his conviction that the United Nations and particularly the security Council, which he deems much better suited for such purposes than the General Assembly, should be effective instruments for achieving such goals.

In providing the reader with a detailed and impartial account of proceedings before the UN, Ambassador Lall displays admirable objectivity. His impartiality is particularly noteworthy because the kind of experience that he has had in Middle East affairs often produces an anti-Israeli bias. For more than two years he served as India's representative on the General Assembly's Advisory Committee which was charged with assisting the Secretary-General in directing the work of the United Nations Emergency Force. Israel's refusal to permit such forces on her territory (in contrast to Egypt's affirmative response over a period of ten years) is duly noted, but the author's ascription of even larger obligations on Egypt's part offers no support for simplistic defenses of the latter's decision to order the withdrawal of UNEF in May, 1967.

This study would be of greater value to the average reader if it contained even a brief account of the roots of the conflict, and it would be much enriched by the inclusion of analyses of the interests and goals that lay behind the positions taken by the parties to the dispute and by the other principal actors at the UN. Equally regrettable is the absence of any attempt to evaluate the disputants' charges and countercharges concerning breaches of the peace and acts of aggression. However, those interested in bargaining or in the utility of international organization will be grateful for Ambassador Lall's identification of lost opportunities for peaceful settlement and his catalog of institutional failures, such as the Advisory Committee's failure to call the General Assembly into special session when Egypt ordered the withdrawal of UNEF prior to the "six-day war." Moreover, his legal-political analysis of the status and the withdrawal of UNEF (Chapter II) is one of the most valuable parts of the book.

Ambassador Lall's study provides many other insights into the essentially legal aspects of UN procedure. Some examples are noteworthy: (1) the continuing and, to Lall, untenable claims of "belligerent rights" by all parties to the conflict *vs.* obligations under the Charter to refrain from the threat or use of force; (2) the alleged legal incapacity of the UN to "condemn" states as aggressors, a limitation vigorously defended by the author in his critique of certain clauses of anti-Israeli resolutions proposed by the Soviet Union and other "pro-Arab" states; and (3) the ease with which the organization sometimes functions without regard to legal technicalities. In regard to the latter point, it appears that the special session of the General Assembly was called by the Secretary-General in June, 1967, without regard to established procedures but simply on request of the Soviet Union. In accordance with Soviet intent, moreover, the Assembly proceeded to consider draft resolutions (two of which were eventually passed) without regard to Article 12 of the Charter, which bars Assembly recommendations concerning disputes that are being considered by the Security Council. As Ambassador Lall observes, there are several precedents for this deviation from the Charter. With regard to Security Council voting procedure, however, the Middle East crisis produced real innovations. During a time of alleged cease-fire violations, and while the Council was deadlocked in debate over draft resolutions concerning the substance of the entire dispute, the President of the Council proclaimed "consensus" in favor of stationing UN Military Observers in the Suez Canal sector. There was no objection, and he declared that the consensus was accepted. Shortly thereafter (again without objection) he referred to this consensus as "the *decision* which we have just adopted." (See p. 202.) More innovation, however, occurred during a similar impasse later in the year when another President of the Council consulted with his colleagues and proclaimed a decision in their name. Ambassador Lall sums up this development as follows:

> This was the first time in the Council's history that a substantive decision was taken by a consensus arrived at without a formal meeting being called. By doing without a meeting the Council both avoided contention and saved face for certain of its members. (P. 268.)

This decision authorized the Secretary-General to enlarge the number of observers in the Suez Canal region and to arrange for additional technical material and means of transportation.

During the last six months of 1967, the General Assembly passed two resolutions dealing with rather limited aspects of the Middle East crisis, while the Security Council produced seven resolutions (one of which, on November 22, addressed itself to all the issues in dispute and provided an "authoritative" outline of basic principles for a general settlement). These actions were in addition to its two other "decisions" arrived at by "consensus." Two years later, however, the substantive terms of all these recommendations and directives remain unimplemented, in spite of the passage of many subsequent resolutions reaffirming the UN's previous actions. In defiance of the UN, Jerusalem has been incorporated into the Republic of Israel, no captured territory has been evacuated, ceasefire violations occur daily and *de facto* warfare is recurrent, there is no settlement of the refugee problem, freedom of navigation on international waterways remains to be established, and both sides in the conflict claim the rights of belligerents. The gap between success in negotiating the passage of resolutions and securing their implementation continues to widen, and the attempts at quiet mediation by the UN's skilled Special Representative to the Middle East have been unavailing.

In this reviewer's opinion, the fundamental conflict between Arab nationalism and Israel's assessment of her own basic security requirements makes clear that the only peace that is possible would be one enforced by a Soviet-American consensus, whether imposed under UN auspices or otherwise. Since both states lack the will to impose a settlement—even when agreeing on one, as they did in 1967 (see pp. 211, 263, 309)—the Middle East will continue indefinitely to be an arena of armed conflict in which minor powers have the means to involve the great powers in a "war by proxy" or in direct conflict. Even an imposed settlement, however, might entail prohibitive risks and costs. This gloomy forecast, it should be noted, is in sharp contrast with the position taken by Ambassador Lall following passage of the Security Council's comprehensive resolution of November 22, 1967. In his view,

> Never was the prospect of peace in the Middle East brighter than at the beginning of 1968. The chances of moving forward from the armistice regime instituted nineteen years previously were good and solid. Only grave errors on the part of the international community could fail to convert this opportunity into a reality. (P. 279.)

Such conclusions seemed unwarranted even in their original context, since Ambassador Lall accurately underscored the resolution's ambiguities and also reported that none of the parties to the conflict committed itself to carry out the resolutions provisions. In the case of Israel, for example, Foreign Minister Eban declared: "My Government will determine its attitude to the Security Council's resolution in the light of its own policy . . ." (Quoted, p. 264.)—H. PAUL CASTLEBERRY, *Washington State University*.

*German Foreign Policy in Transition: Bonn Between East and West.* BY KARL KAISER. (New York: Oxford University Press, 1968. Pp. 153. $1.95.)

The reader may well expect an enriching reading experience when he first takes this slender volume in hand. The author, a German political scientist who has taught at Harvard, the Johns Hopkins Center at Bologna and the University of Bonn, is a widely published and deservedly respected student of European and German politics. The publisher is a university press of the first rank, and the book was issued under the joint auspices of several prestigious scholarly institutions. However, while the essay contains some useful descriptions of and insights into the roots and implications of recent shifts in Bonn's *Deutschlandpolitik,* such virtues are regrettably buried beneath the verbiage of what is a very poorly written book. Indeed, after three close readings, I am not certain that the values of those scattered paragraphs are commensurate with the chore of wading through such an inconsistently edited, logically flawed and generally disappointing volume.

While the title indicates a rather broad scope, the author demurs: "This study is not a history of German foreign policy of the post-war period, but only an essay. It deliberately focuses on certain themes and does not discuss in detail a number of dimensions . . ." (p. 4). The themes Kaiser has selected are restricted almost entirely to the Federal Republic's policies regarding the division of Germany and to the interplay between those policies and their domestic and systemic environments. He attributes the growth of Bonn's attachment to the West in the years after the war, the development of the Hallstein Doctrine (intended to impose a diplomatic quarantine on the East German regime) and the assumption of the "policy of strength" to a convergence of Western (one could generally read "American") strategic ends and the efforts of West German politicians to cope with their endemic problems of insecurity.

This post-war posture did not manifest potential for bringing reunification closer. The mutually reinforcing (or exacerbating) relationship between the division of Germany on the one hand and the division of Europe and the Cold War confrontation on the other led to an impasse familiar to all observers of the world scene: a *détente* between the blocs was viewed to be a prerequisite for the resolution of the "German problem," and German reunification appeared impossible without a prior relaxation of Cold War tensions. Bonn's stance remained essentially unchanged until its western allies signalled a shift toward *détente,* though many West Germans had recognized that progress toward reunification could not be made through such policies.

Movement away from the rigid post-war posture was slow and difficult, especially until the advent of the Great Coalition, in 1966, because *Ostpolitik* in general and *Deutschlandpolitik* in particular had become highly politicized in the Federal Republic. Further, as Kaiser points out, the unresolved German question translates many German phenomena into significant inputs into the European and global systems. It also magnifies the impact of many external developments on politics inside the Federal Republic.

Since Kaiser is generally thorough in his treatment of environmental factors, his one omission stands out: he fails to take adequate note of the import of the primacy of concern that has come to characterize America's extra-European involvement during the very years that Bonn's reorientation has taken place. While it was the Kennedy administration's move toward the policy of *détente* that gave initial impetus to Bonn's shift, it was America's deepening involvement in Vietnam in 1965 and 1966 that provided the enhanced freedom of action for the Federal Republic that made the policy changes possible. The opening to the East clearly bears a German imprint—and it was contrary to the American style of "alliance leadership" prior to the escalation of the war in Asia to yield that much initiative to its partners.

Kaiser refrains from attempting to resolve the dilemmas borne by Germany's division and unique world position. He does conclude on a reasonably positive note, however:

> West Germany's adaptation to the division and her attempt to enter into a peaceful working relationship with East Germany, though not a solution to the German problem, may still be a net gain. There are no *feasible* alternatives (p. 139).

While in his descriptions of the evolution of Bonn's policies the author has made a valuable contribution to the body of "current historical" knowledge, his analysis in general is marred by logical lapses, extremely distracting language and occasional manifestations of conceptual indifference. Two or three illustrations of each of these problems should suffice here.

On page 54 Professor Kaiser presents four tables, in order to demonstrate that

> a reunified Germany would not . . . simply be the sum of two existing states. She would occupy an entirely new category—that of a nation whose power fell short of the two super-powers but considerably ahead (except for the possession of nuclear weapons) of Europe's traditional great powers like Britain and France (p. 55).

Yet, the elaborate tables demonstrate no such thing! In each the Federal Republic's rank is the same as the rank that a reunited Germany would occupy—and in each the Federal Republic already surpasses the standing of all European powers! On page 48 Kaiser writes that,

> as German critics of past Bonn policies have often pointed out, governments are not pawns in the hands of their public but can influence public opinion through courageous and convincing leadership. *To do so requires, however, that a majority of those are convinced of the wisdom of the move* [italics added].

To move public opinion in the direction of supporting governmental policy does not, of course, require the conviction of 51% of "those in key positions"—or, for that matter, of any specific number or proportion. That task can be achieved by one individual in some circumstances—or the whole leadership can fail in attempting it.

Throughout the text such terms as *nation* and *interaction* are used without particular regard for their established conceptual meanings. Such usage could be viewed as simply another manifestation of Kaiser's monumental language problem were it not for his effort to develop what is a provocative and potentially fascinating analytic line. Kaiser asserts that the two Germanies

> are two parts of one nation and, therefore, taken together, from a *domestic* national context with its own habits of thought and action, quite different from those characteristic of relations between other nations in opposing international systems (p. 28).

In order to use this notion it is imperative to distinguish this view of the domestic context from the more orthodox one—or at least to restrict usage to the former. This is not done; and the term is used in both senses indiscriminately. Second, the concept of nation requires specification if it is to serve purpose to which the author wishes to put it. In spite of the recurring bald assertion that "Germany is still one nation" (at pp. 21, 28f., chap. 8 and elsewhere), the determinants and boundaries of the concept are never made clear.

The most distressing aspect of this book is the language. The syntax is a jumbled mixture of English and German. Very few sentences flow smoothly, and graceful construction is not manifested at all. Subordinate clauses frequently dangle in grotesque ways. The vocabulary is extremely narrow, leading to banal formulations of good thoughts and to a general sense of *déjà vu.* It is this last characteristic that makes the book read long. Finally, some uncorrected grammatic errors remain in the text. Perhaps Kaiser should have written in his native language and then had the work translated.—MARTIN O. HEISLER, *University of Maryland.*

*Eleven Against War,* BY SONDRA R. HERMAN. (Stanford: Hoover Institution Press, 1969. Pp. 264. $2.95, paper.)

*Eleven Against War* is an impressive study of the extension of American reform sentiment into the arena of foreign affairs. The author wishes to generate a deeper understanding of the resulting international idealism which, she believes, received too hasty a burial by realistic thinkers (Kennan, Morgenthau, Nieburh) in the Cold War Era. To accomplish her purpose, Herman explores the views of eleven American reformers: Woodrow Wilson, Elihu Root, Nicholas M. Butler, Hamilton Holt, Theodore Marburg, A. Lawrence Lowell, John Bates Clark, Franklin H. Giddings, Josiah Royce, Jane Addams, and Thorstein Veblen. All were dedicated to changing the relationships of nation states with the avowed aim of preventing future wars. The life experiences of the reformers, their basic philosophical assumptions, social and political theories, and international conceptions are lucidly presented and interrelated. Each chapter is rich in information and skillful in synthesis.

The major thesis of the book is that American internationalism in the early twentieth century contained a sharp division between the "men of the polity" and the "communalists." The former (Wilson, Root, Butler, Holt, Marburg, Lowell, Clark, Giddings) extended their atomistic, competitive view of American society into the international arena. A natural harmony of interests was assumed to exist between the advanced nation states. Free competition among these nations would be beneficent if protected and tamed by the rule of international law. The "men of the polity" tended to be international Spencerians, expecting Anglo-American influence to dominate, not by force, but by the persuasion of a superior economic and legal system. Accordingly, their proposals for international peace stressed legal and judicial mechanisms for maintaining non-violent trade rivalries. Within this consensus, they differed among themselves concerning the nature of sanctions to be applied when a nation refused to respect international procedures.

The "communists" (Royce, Addams, and Veblen) flatly rejected competitive individualism as a sound guide for national or international society. They believed that peace could only be achieved by *creating* an organic community embracing all the peoples of the world. Their proposals emphasized schemes of economic and social cooperation which they hoped would gradually corrode the very spirit of competitive nationalism. The author is generally convincing in demonstrating the connection between the domestic and international views of the individuals considered. The exception is Hamilton Holt who emerges as a strange hybrid when placed in the author's conceptual framework.

Herman credits the "communalists" with recognizing that no natural harmony of interests existed between the advanced nations, and that the nation state system itself created conditions injurious to harmony. The major shortcoming of these thinkers, she contends, was their remoteness from the large organizations which could influence and execute public policy. Jane Addams, although an able leader of a minority protest movement (The Women's Peace Party), never held a public office higher than garbage inspector for the nineteenth ward of Chicago until late in her career. Herman's treatment of Veblen reinforces his image as a lone wolf howling bitterly and brilliantly at the moon. Royce was also detached from large influential organizations.

Herman concludes that the two branches of international idealism failed to recognize their supplementary character. By arguing in nationalistic terms, "men of the polity" made no attempt to educate the public to the possibility of cooperation within a world community. "Communalists," on the other hand, failed to recognize that new cooperative departures could not be made without the participation of large, influential, and established organizations. The responsibility for the decline of internationalism, therefore, rests fundamentally with the internationalists themselves for "cherishing their differences."

The author's conclusion lacks persuasiveness because it does not come to terms with the vast practical problems which faced both the "men of the polity" and the "communalists" in their search for world peace. She wisely prefaces her book with the remark that is is essentially a study of ideas which touches only briefly on why, in the end, the internationalists failed. Judged in the light of its primary objective, *Eleven Against War* succeeds admirably.—EDWARD N. KEARNY, *Western Kentucky University.*

*How Nations Behave.* BY LOUIS HENKIN. (published for the Council on Foreign Relations) (New York: Frederick Praeger, 1968. Pp. 324. $7.50.)

Louis Henkin, a competent and respected international lawyer, seeks to explain the relevance of international law to contemporary world events. To this end, he undertakes to answer the question: "in what ways does international law determine or govern or modify, the policies of governments and how nations behave toward each other." Henkin reserves the right to engage in occasional skirmishes with other international law writers. However, the book is primarily addressed to the general reader who is interested in international relations but who underestimates or is unaware of the influence of international law in international political behavior.

This is a worthwhile enterprise. Unfortunately, the author does not do justice to the subject and the book falls short of its goal. The book fails because the central issue of the influence of international law on "international behavior" is not adequately met. Arguments, extrapolations from other legal systems, and speculation about the content and causes of international behavior are presented in a way that fails, in my view, to support persuasively the contention that "international law does far better than its reputation." This is a pity, since the contention is probably true. Indeed, most of Henkin's contentions are plausible and acceptable to those who are familiar with the subject. But the object of the book is to educate and persuade those who are less familiar with or skeptical of international law. This requires more thorough argumentation and evidence, as well as more penetrating scholarship, than is reflected in *How Nations Behave.*

For example, Henkin states that, "A government contemplating action usually asks what the law has been, independently of what it proposes to do." This is quite possibly true. But little evidence is offered in support of this contention. Moreover, there is no adequate investigation of the possibility that, even if the contention is valid, its significance is not as favorable to Henkin's defense of international law as he seems to assume. Thus, the search for relevant international law within the policy process may be made primarily in order to anticipate and neutralize legal obstacles. Or the search for law may be a search for legal justifications for doing things that the government intends to do in any event. Percy Corbett pursued these themes in devastating detail in LAW IN DIPLOMACY (Princeton University Press, 1959) and Corbett was a friend and practitioner of international law. Surely the uninitiated and the skeptics may be expected to think of them and it may be questioned whether they will be convinced by Henkin's optimistic treatment of the subject.

Henkin attempts to protect himself from the charge of superficiality by stating at the outset that his exploration of the subject "must be *a priori* and speculative, less scientific than impressionistic . . . The examples cited are few, and many of them come from the United States." Fair enough, but even by his own standards it appears that Henkin does not provide convincing evidence to support his position and to win over the readers to whom the book is directed.

Specifically, three attempts to make a case for international law fail for lack of evidence. First, the valid and familiar point is made that much of the influence of international law within governmental processes is unknown and, in many cases, unknowable, to the outside world. Granted the problems of access to evidence of such influence, confidentiality and classification, a scholar of Henkin's stature should be able to provide numerous concrete examples of international law at work. They could be taken from memoirs and biographies of statesmen, diplomatic history, and from the author's personal experience as well as the experiences of professional colleagues. Instead, Henkin gives a few *hypothetical* examples of international law being applied. This is not even an "impressionistic" sampling of behavior. It is no sampling. The reader is apparently expected to take Henkin's word that international law influences decision-making in a way analagous to that described in the hypothetical examples. Henkin may be substantially right but his book is about *how* nations *behave* and this portion of the book is not likely to persuade readers that the author's hypotheses correspond to reality.

Second, Henkin attempts to rebut the claim that adherence to international law is largely limited to occasions when national interests coincide with legal prescription. To accomplish this he takes two very curious examples: loyalty to alliance commitments at the outbreak of World War I and within NATO. In each case Henkin points out that performance of legal obligations has at times required the doing or contemplation of things that would not appear to be in the national interests of some of the allies. No allusion is made to the political, economic, and military factors—particularly as packaged in long-range contingency plans—which may have been more important by far than legal obligations in determining the behavior of the alliance members. Henkin follows these two examples with a third concerning reaction to Hitler's invasion of Poland. It seems to me that the examples chosen fail sufficiently to meet the challenge taken up, namely of proving that law can prevail over national interests.

Henkin also attempts something that is quite important, namely analysis of "the law's other influences." By this he means the subtle role that legal concepts may play, along with many others, in decision-making. His example is excellent, the U.S. handling of the Cuban Missile Crisis. But his analysis is disappointing. Thus, in arguing against the possible justification of anticipatory self-defense he supports his negative reaction to this concept with one footnote to an article by Quincy Wright and with a full page quotation of himself, surely a most unusual practice, supplemented by some more footnotes to his own writing. On this highly relevant and controversial subject, in a book designed to impress general readers, such short-cuts detract from the legitimate points made and from the authority of the writer.

In short, the much-needed book which Professor Henkin set out to write is still to be written.—WILLIAM V. O'BRIEN, *Georgetown University*.

# ANNOUNCEMENTS

## ADDRESS FOR NEW MANUSCRIPTS AFTER JUNE 1, 1970

Beginning June 1, 1970, the REVIEW's Managing Editor Elect, Professor Nelson W. Polsby, will assume all responsibility for referring and making decisions on all newly submitted manuscripts and for deciding on manuscripts referred but not decided on prior to June 1. Accordingly, after June 1, 1970 all new manuscripts and all correspondence concerning manuscripts not then decided upon should be sent to:

Professor Nelson W. Polsby, Managing Editor Elect
*American Political Science Review*
Department of Political Science
University of California, Berkeley
Berkeley, California 94720

Each manuscript should be accompanied by an abstract of up to 200 words briefly describing the article's contents. All manuscripts and abstracts should be submitted IN DUPLICATE. They should be double-spaced and may be in typed, mimeographed, hectographed, or other legible form. Footnotes should appear at the end of the manuscript, not at the bottom of the page.

Since manuscripts are sent out anonymously for editorial evaluation, the author's name and affiliations should appear only on a separate covering page. All footnotes identifying the author should also appear on a separate page.

## THE SELECTION OF NELSON W. POLSBY AS THE NEW MANAGING EDITOR

On November 14, 1969, the present Managing Editor submitted his resignation to President Karl W. Deutsch. The letter said, in part, "A word seems in order about the timing of [the resignation]. In my discussions with then-President David Truman I agreed to undertake the editorship for a period of three to five years. The transition period began in July, 1965, and the first issue published under my direction was that of March, 1966. Accordingly, I am currently in my fifth year of service and feel that I have fulfilled my agreement with Dave. Quite aside from this, however, it has always seemed to me that a fairly frequent rotation of editors is healthy for the journal (and the editors) and that five years is a reasonable maximum tenure."

The Constitution of the Association (Article VI, section 1) provides: "The . . . Managing Editor of *The American Political Science Review* shall be appointed by the Council, after it hears the recommendation of the President. [He] shall have [a term] to be fixed in each case by the Council; and [he] shall be eligible for reappointment."

At its meeting of December 4-5, the Council established a Search and Screening Committee to assist President Deutsch in considering candidates for the new Managing Editor. The Council stipulated that the Committee be composed of the President as Chairman, President Elect, Managing Editor, three members of the Council, and one member of the current Editorial Board. To the four appointive positions President Deutsch named Herbert McClosky, University of California, Berkeley; Joseph L. Nogee, University of Houston; Vernon Van Dyke, University of Iowa; and Robert E. Ward, University of Michigan.

On December 8 the Executive Director sent a letter to all department chairmen advising them of the vacancy and asking for their suggestions and nominations for filling it. A total of twenty-two replies were received in time to be considered by the Committee. The Council directed the President to recommend a person for the editorship at its next meeting.

Six of the seven Committee members met in Cambridge, Mass. on December 23. Professor Ward was unable to attend but communicated his views to the President in advance of the meeting. At the meeting the Committee reviewed all the persons—some ninety-four names in all—who had been suggested to them. Several names were eliminated from further consideration because of information, gained from informal soundings, of their wishes not to be considered.

For evaluating those remaining, the Committee agreed upon the following main criteria. First and foremost, the new Managing Editor should be committed to encouraging the best work done by all of the different approaches to political analysis now used or emerging in the discipline; he or she should not be dedicated in any way to promoting any particular approach over the others. The new editor should be a scholar of established reputation. He or she should have editorial talent for evaluating and improving manuscripts as well as administrative talent for managing the journal's office and communications. The editor should already have demonstrated his or her possession of these talents in some way—e.g., by having edited another journal or a volume of essays, or by having acted as editorial advisor for a publisher. The editor should have shown ability both to apply high standards impartially in deciding what to publish and to help authors to present their work in the clearest and most readable form. The editor should be widely familiar with the profession's active scholars, both those older and established and those younger and rising, in all the nation's regions. He or she should be knowledgeable in American Government and Political Theory, since a majority of the manuscripts submitted are in these fields; but it would be highly desirable for the editor also to have some competence in Comparative Politics and International Politics. And it would also be desirable for the editor to have some competence in related disciplines and some experience in interdisciplinary research and writing.

After agreeing on the foregoing criteria, the Committee proceeded to consider the ninety-odd remaining nominees. After much discussion the number was reduced to a "short list" of six. On a secret ballot each Committee member then ranked the six names in his order of preference. The preferences on the six ballots were combined into a composite ranking, and the Committee agreed that the intervals among the top names were sufficiently large to request the President to present the first name to the Council for its approval.

At the meeting of the Council in Cambridge on January 14 President Deutsch explained the Committee's criteria and procedures and expressed the members' regret that only one of so many excellent nominees could be chosen. He then presented the Committee's first choice: Professor Nelson W. Polsby of the University of California, Berkeley. After some discussion the Council unanimously approved the President's recommendation and Professor Polsby was named the next Managing Editor of the REVIEW. He has since accepted the appointment, and will assume its full duties in 1971.

## ERRATA

The Officers' Page of the December, 1969 issue incorrectly listed the institutional affiliation of Council Member Allan P. Sindler as the State University of New York, Stony Brook. Professor Sindler's correct affiliation is the University of California, Berkeley.

Also in the December, 1969 issue, the second sentence of the first paragraph on p. 1216 of Jeff Fishel's article on "Party, Ideology, and the Congressional Challenger" should read: "Of 432 non-incumbents running in that election, 70 percent completed and returned the questionnaire, a response rate sufficiently high to justify the analysis undertaken here."

## ARTICLES ACCEPTED FOR FUTURE PUBLICATION*

*June, 1970*

Joel D. Aberbach and Jack L. Walker, University of Michigan, "The Meanings of Black Power: A Comparison of White and Black Interpretations of a Political Slogan"

Steven J. Brams, New York University, and Michael O'Leary, Syracuse University, "An Axiomatic Model of Voting Bodies"

Steven R. Brown and John D. Ellithorp, Kent State University, "Emotional Experiences in Political Groups: The Case of the McCarthy Phenomenon"

Walter Dean Burnham and John D. Sprague, Washington University, St. Louis, "Additive and Multiplicative Models of the Voting Universe: The Case of Pennsylvania, 1960–1968"

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* Production exigencies may occasionally necessitate publication of articles in issues other than those given in this listing.

William R. Caspary, Washington University, St. Louis, "The 'Mood Theory': A Study of Public Opinion and Foreign Policy"

Otto A. Davis, Melvin J. Hinich, and Peter C. Ordeshook, Carnegie-Mellon University, "An Expository Development of a Mathematical Model of Policy Formation in a Democratic Society"

Ada W. Finifter, Michigan State University, "Dimensions of Political Alienation"

Bryan R. Fry and Richard Winters, Stanford University, "The Politics of Redistribution"

M. Kent Jennings, University of Michigan, and L. Harmon Zeigler, University of Oregon, "The Salience of American State Politics"

Norman N. Miller, Michigan State University, "The Rural African Party: Political Participation in Tanzania"

Paul E. Peterson, University of Chicago, "Forms of Representation: Participation of the Poor in the Community Action Program"

David E. Powell and Paul Shoup, University of Virginia, "The Emergence of Political Science in Communist Countries"

John Duncan Powell, Harvard University, "Peasant Society and Clientelist Politics"

*September, 1970*

Michael Aiken and Robert R. Alford, University of Wisconsin, "Community Structure and Innovation: The Case of Public Housing"

Gordon S. Black, University of Rochester, "Some Notes on the Process of Professionalization in Politics"

Jack Dennis, University of Wisconsin, "Support for the Institution of Elections by the Mass Public"

John P. Diggins, University of California, Irvine, "Ideology and Pragmatism: Philosophy or Passion?"

Melvin J. Hinich and Peter C. Ordeshook, Carnegie-Mellon University, "Plurality Maximization vs. Vote Maximization: A Spatial Analysis with Variable Participation"

Barbara Hinckley, University of Massachusetts, "Incumbency and the Presidential Vote in Senate Elections: Defining Parameters of Subpresidential Voting"

Raymond F. Hopkins, Swarthmore College, "The Role of the M.P. in Tanzania"

Chong Lim Kim, University of Iowa, "Political Attitudes of Defeated Candidates in American State Elections"

Edward N. Muller, University of Iowa, "Cross-National Dimensions of Political Competence"

Donald Rothchild, University of California, Davis, "Kenya's Africanization Program: Priorities of Development and Equity"

John W. Soule and James W. Clarke, Florida State University, "Amateurs and Professionals: A Study of Delegates to the Democratic National Convention"

Gilbert R. Winham, McMaster University, "Political Development and Lerner's Theory: Further Tests of a Causal Model"

*December, 1970*

Joel D. Aberbach and Jack L. Walker, University of Michigan, "Political Trust and Racial Ideology"

Hubert M. Blalock, Jr., University of North Carolina, "A Causal Approach to Nonrandom Measurement Errors"

Giuseppe Di Palma and Herbert McClosky, University of California, Berkeley, "Personality and Conformity to Political and Social Attitudes"

Harlan Hahn, University of California, Riverside, "Correlates of Public Sentiments on War: The Vietnam Referendums"

Henry S. Kariel, University of Hawaii, "Creating Political Reality"

Robert Melson, Michigan State University, and Howard Wolpe, Western Michigan University, "Modernization and the Politics of Communalism: A Theoretical Perspective"

Edward N. Muller, University of Iowa, "The Representation of Citizens by Political Authorities: Consequences for Regime Support"

Eric Nordlinger, Brandeis University, "Soldiers in Mufti: The Impact of Military Rule upon Economic Change in the Non-western States"

Giovanni Sartori, University of Florence, "Concept Misformation in Comparative Politics"

Herbert F. Weisberg, University of Michigan, and Jerrold G. Rusk, Purdue University, "Dimensions of Candidate Evaluation"

Jerzy J. Wiatr, University of Warsaw, "Political Parties, Interest Representation and Economic Development in Poland"

# ANNOUNCEMENT OF NOMINATING COMMITTEE APPOINTMENTS

## For Selection of Officers of the American Political Science Association

## For 1971-72

Officers to be selected: President-Elect, three Vice Presidents, Secretary, Treasurer, eight Members of the Council for two-year terms. Al lmembers of the Association are invited to submit suggestions to the Chairman or other members of the Nominating Committee.

J. David Singer, University of Michigan, Chairman
Hayward R. Alker, Jr., Massachusetts Institute of Technology
Frederic N. Cleaveland, University of North Carolina
Duncan MacRae, Jr., University of Chicago
John C. Wahlke, University of Iowa
Sheldon S. Wolin, University of California, Berkeley

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# TRAVEL GRANTS

# IPSA WORLD CONGRESS

## Munich, Germany

## August 31 to September 5, 1970

The Association has received a grant of $10,000 from the National Science Foundation to assist American political scientists in travel to the International Political Science Association Congress in Munich, Germany, August 31 to September 5, 1970. Grants are limited to travel costs.

Application forms are available from the Association office. The deadline for receipt of applications will be June 1, 1970. An Association committee will make the grants and notify individuals soon after that date.

Write to: Travel Grants, IPSA Congress
American Political Science Association
1527 New Hampshire Avenue, N. W.
Washington, D. C. 20036

# EUROPEAN FLIGHTS

## TRAVEL-STUDY OPPORTUNITIES

For the eleventh successive summer, the Association is arranging two flights to Europe in 1970. Flight A will leave New York June 10 for London, and return September 7. The A Flight will be by Pan American and will cost $245.00. Flight B will leave New York for Paris August 1 on Pan American, and will return September 7. The cost for Flight B will be $270.00.

Reservations are being accepted on a "first come, first served" basis. These flights are open only to members of the Association and their immediate families. Contact the Association for further information.

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The American Political Science Association
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**8th World Congress, August 31 to September 5th, 1970**

**Munich, Germany**

Members of the American Political Science Association are invited to participate in a group flight to Munich, Germany leaving JFK International Airport in New York August 22 and returning September 7, 1970. The fare will be $245.00 on Lufthansa German Airlines.

Persons wishing to make reservations now may write directly to the Association, in care of the Director, Summer Flight Programs, 1527 New Hampshire Avenue, N. W., Washington, D.C. 20036.

# GROUP FLIGHTS TO LOS ANGELES

## for 1970 APSA Annual Meeting

## September 8-12

The association has arranged group flights from several cities to Los Angeles for the APSA Annual Meeting, September 8-12, 1970. The group fares are the same as excursion fares, but the regulations will permit individuals to return any day of the week and in less than seven days.

The fares for these flights are based on a minimum of 25 participants on regularly scheduled airlines:

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The American Political Science Association

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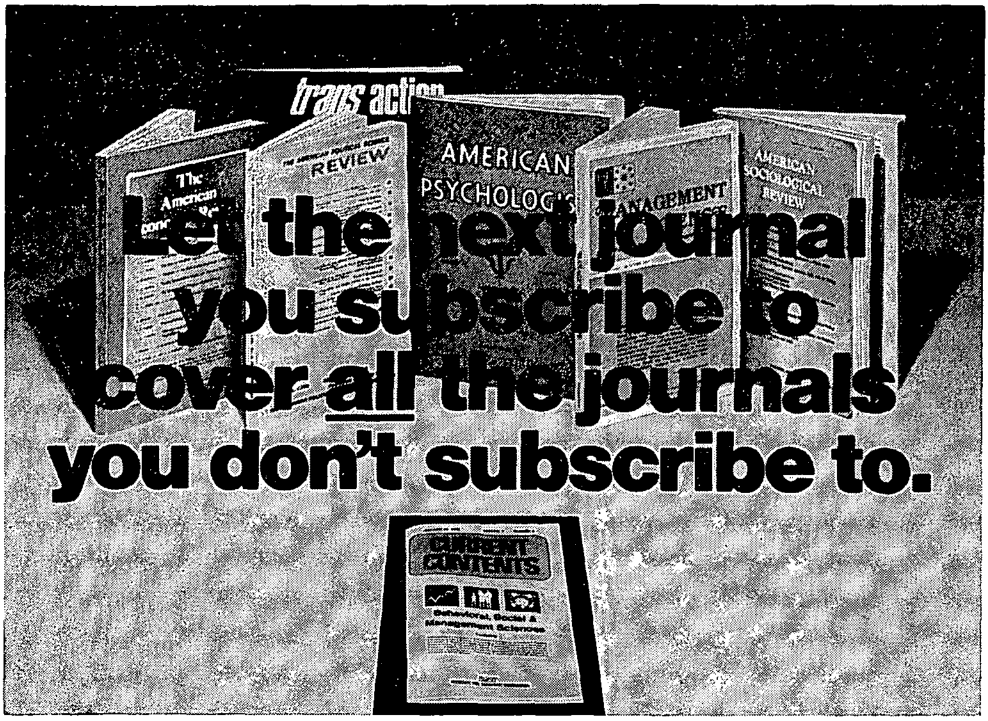
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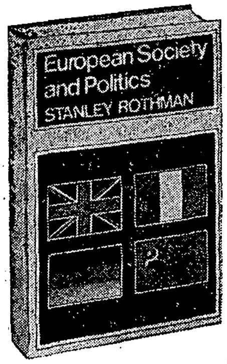

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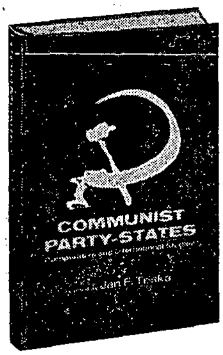

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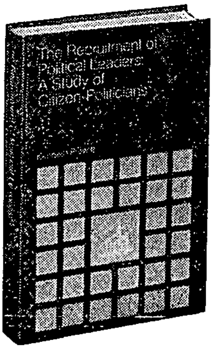
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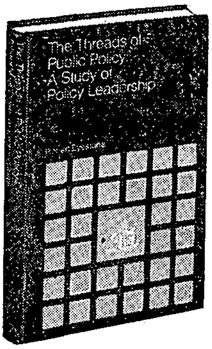

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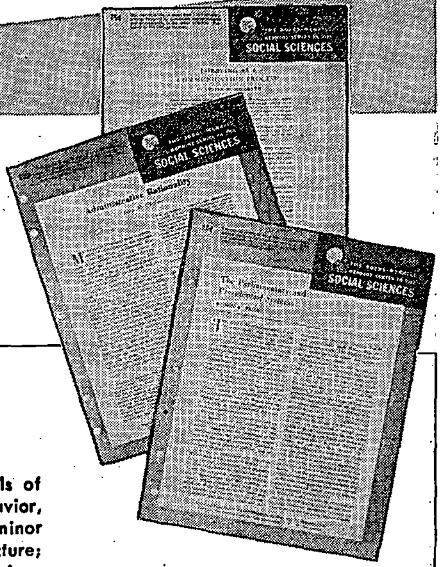

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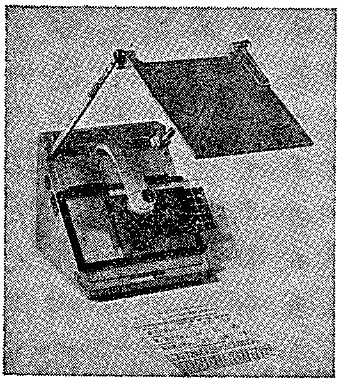

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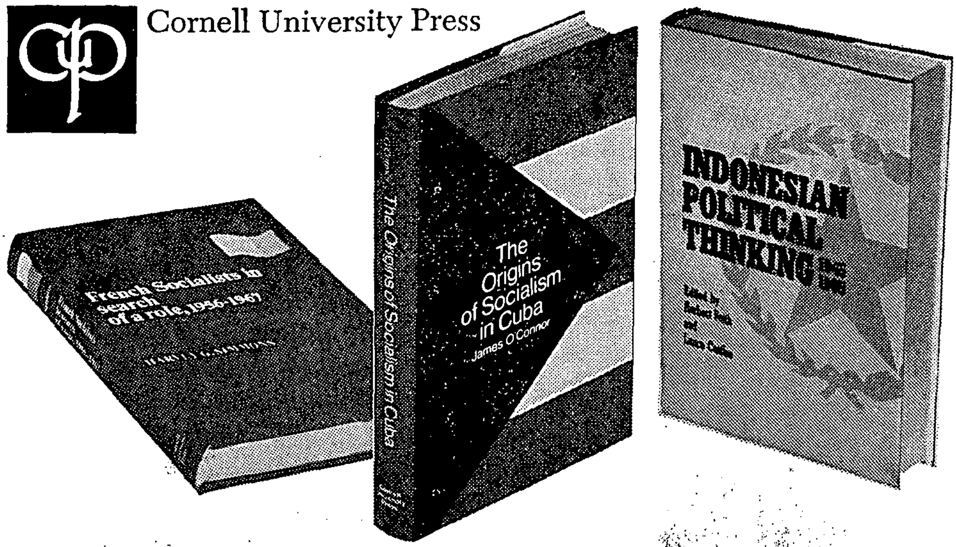
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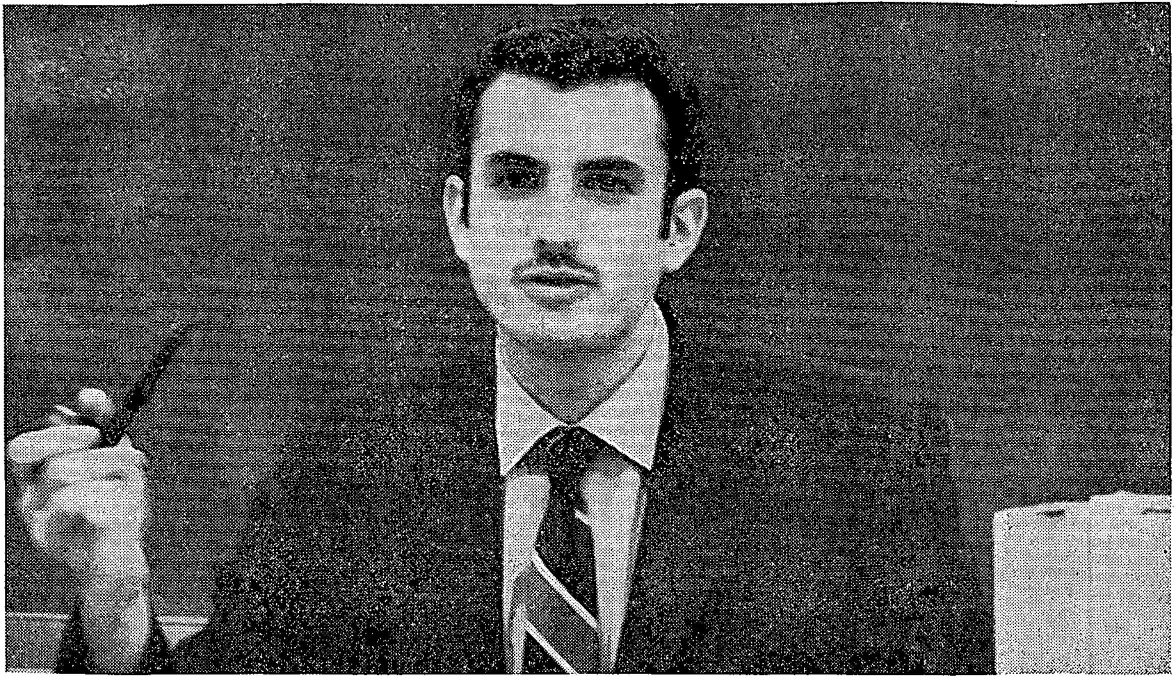

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